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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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A STUDY OF PSYCHOKINESIS IN ITS RELATION TO PHYSICAL CONDITIONS

By H. FORWALD

ABSTRACT: In the experiment reported here, the author makes an important advance over the work he has already published in the Journal on the PK placement effect, that is, psychokinetic action causing neutrally released cubes to roll to a designated half of a table. In a preceding experiment he introduced the practice of measuring the position of the cubes on a table top ruled in centimeters and thus obtained the average distance from the central dividing line when the A-side was target and the average distance when the B-side was target. The difference was used in measuring the amount of PK effect exerted. He went still further and developed a formula for the evaluation of the amount of force (in dynes) required to move a cube of the weight used in his experiment one centimeter laterally in the test. And there have been other developments.

Now in the new work three physically different kinds of bakelite cubes were used in comparative tests, two kinds being released in a group at the same time. The three types differed in roughness of surface, one being smooth, another medium rough, and the third very rough. The question behind the experiment was whether the physical conditions of the test object would exert any influence on the scoring rate. No significant results were obtained when the smooth and the medium rough cubes were thrown together. But when the very rough and the smooth cubes were thrown together, the very rough cubes gave a significant result.

A number of incidental findings are reported; for example, apparently the smooth cubes, when they were released with the very rough ones, were influenced in a direction opposite to that which the subject intended. This was most marked, as PK usually is, on the first throw of a given set. On this first throw the results with the very rough cubes were highly significant and they were also significantly different from the smooth cubes thrown at the same time. Using this first throw as a basis for computation, an estimate was made of the force required for the lateral displacement and it was found to be considerably greater on the rough cubes than on the smooth ones. A comparison was made between the average of these two types of bakelite cubes and wooden cubes used in an earlier series under similar conditions. The average of the bakelite cubes closely approximates that of the wooden ones in numbers of dynes.

The author presents the research against a background of psychophysical theory and discusses the findings in relation to the physical conditions of the

experiment. He interprets his findings as indicating the influence on the end results of certain physical factors in addition to the PK factor itself, and he identifies his approach as a positivistic one.—Ed.

Introduction

THE PRESENT ARTICLE is a report on some new experiments in psychokinesis (PK), in which attempts were made to approach the problem of whether the results in placement experiments with dice indicate any dependence on the physical conditions prevailing in the experiments. The principal object of the investigation is the comparison of the PK effects on cubes of different weight and roughness of surface. Before dealing with the actual experiments it seems useful, however, to discuss some general considerations which may facilitate the appreciation of the results on a recognized physical basis.

According to the glossary in this *Journal* "psychokinesis" means "the direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation." It could be added that the "direct influence" is considered to be the result of a *mental* action.

The process involved is strange to physical science because the idea that something going on in a human mind can have any influence on inanimate matter is not accepted. In this connection it is interesting to quote Schrödinger (**7**; p. 53) in his objections to the opinion of Bohr and Heisenberg on the uncertainty relation: "Since the observing mind is not a physical system, it cannot interact with any physical system."

This is a natural opinion because it is a result of general experience. But whether or not such a statement holds true under all circumstances is a question which apparently has not been empirically investigated in physics. There seems to be a widespread opinion among physicists that an investigation of this kind is practically impossible to carry out. In this connection Planck (**6**; p. 244) made the following statement in a discussion on free will in his lecture "Positivismus und reale Aussenwelt" (1930): "The object, and the subject of the process of knowing can never be identical, because one can really speak of knowledge only in case the object to be known is not influenced by the processes within the knowing subject."

I do not think that Planck had dice experiments in mind when he made this statement. It must, however, be assumed to apply to inquiries of this kind also. But considering that in dice experiments, when it is specified in advance that a certain effect is looked for and this effect then really appears in the results, this has to be registered as "knowledge" notwithstanding that under the experimental conditions there must have been an influence from the subject (the observing mind) on the object (the cubes).

The results of quantitative experiments in PK have often been considered in the light of quantum mechanics. It has been suggested that a mental control of the rolling cubes could take place in their states of equilibrium when they happened to be on an edge or corner and could thus get them to enter a "favorable causal track" (**5**). It is assumed that in this case a force of but microphysical magnitude would be necessary for the control. The mechanical laws of nature then would not be violated, since macrophysical causality would be maintained.

Apparently there could be a case for this interpretation if one accepts the view of Bohr and Heisenberg as set forth by Schrödinger (**7**; p. 50): "We are to understand that we never observe an object without its being modified or tinged by our own activity in observing it. We are to understand that under the impact of our refined methods of observation and of thinking about the results of our experiments, that mysterious boundary between the subject and the object has broken down."

The German physicist Pascual Jordan (**4**; pp. 141-42) goes a step further. In dealing with dice experiments, he admits the possibility that the rolling dice may not strictly follow the mechanical laws of motion, and he links this idea up with the impossibility of a continuous registration of the physical process of movement in such experiments. This sounds somewhat curious, and from a positivistic point of view the hypothesis apparently is meaningless, because it bears in itself the assumption that it cannot be tested. However, it has to be borne in mind that this hypothesis was established in connection with the recording of die faces. In such experiments there are no means of investigating the dynamics of the movement of the dice. The hypothesis is in a more favorable position when it is considered in connection with the recent placement experiments

which allow for calculations of the dynamics involved (1). If it can be proved that there must have been a physical side force acting on a rolling cube aggregate in order to account for an observed aberration, then it must be concluded that the mechanical laws of motion have not been strictly followed by the cubes.

According to the author's opinion, it is better not to consider at present the hypothesis of a microphysical force acting on the die at the instant of unstable equilibrium, mainly on the ground that the hypothesis can scarcely be tested. But it is also very unlikely that the aberrations in dice experiments can be explained in this way. A microphysical force is an extremely small force, probably of an order comparable, for instance, with the mechanical pressure of a small light beam. The tipping over of a cube by such a small force would require a position very close to that of equilibrium, and the question is whether there would be a sufficient number of such positions (if any) during the time the cube traveled its course. This is a matter which perhaps could be deduced statistically from the observed distance of movement and the corresponding number of rolling turns.

At the present state of the research it seems to be equally possible to consider psychokinesis from a positivistic or from a realistic angle. Nevertheless, for the investigation of PK in relation to a recognized physical basis, a positivistic procedure is at present to be recommended. For convenience, a physical *measure* should be established which permits a reliable comparison of the results under varying experimental conditions. A suitable measure appears to be provided in the force required to give a moving cube aggregate a certain aberration. Such a measure is quite in accordance with usual physical experimental procedure. As mentioned, it represents a positivistic procedure, but it is safer at present to follow such a procedure. As research goes on and we penetrate deeper into the psychokinetic problem, we will probably be forced to move toward a more realistic position (as compared with the present point of view). But it seems to be too early now to introduce explanatory considerations into the psychokinetic complex because we still know so little about the phenomenon that any theory which can be offered must be considered more or less as guesswork. This only leads to theories which cannot at present be proved. But the advancement of science is dependent on hypotheses which can be investigated ex-

perimentally. This kind of careful proceeding is without doubt the most promising way in so difficult a matter as the present. We have to build up our knowledge gradually, at no stage losing contact with established physical facts.

Apparatus and Procedure

In this new placement experiment the author was the only subject and he did his own recording. The apparatus,[1] as before, consisted of a dice table marked off in centimeters longitudinally and transversally (the *X*- and *Y*-directions, respectively) so that the cube positions could be measured. Six cubes at a time were released electrically from a V-shaped container onto an incline from which they entered the table at one end midway between the two sides. From that point they scattered over the surface. The section on the right of the midline as viewed from behind the release mechanism was called the *A*-side; the other, the *B*-side.

The experimental unit, the set, comprised five releases in which the subject tried to get the dice to go to the *A*-side of the table, followed by five releases in which he tried to get them to go to the *B*-side. A set thus consisted of 60 trials, or single die-falls. One series comprised five such sets, or a total of 300 trials.

After the cubes had come to rest, readings were taken in the direction transverse to that of the movement imparted by the incline; i.e., in the *Y*-direction. The plane has a width of 80 cm., and it has ink lines for each centimeter, numbered from 0 to 80, line 0 being at the outer edge of the *A*-side of the table. The container is arranged so as to get the cubes to enter the plane along the 40 cm. line.

The evaluation is based on the difference between the averages for *B*- and *A*-targets. A positive difference indicates a result in the expected direction. On the basis of a Student's *t* test applied to the distribution of individual die positions, it can be found whether the difference is of significant magnitude; i.e., whether there is evidence, considering the conditions under which the experiment was done, that PK forces have acted in the experiment.

The previous experiments with the same apparatus and procedure were carried out with six wooden cubes per release, and the

[1] Described in "An Approach to Instrumental Investigation of Psychokinesis," in this *Journal*, December, 1954.

aberrations were of outstanding significance. The mean side force necessary to cause the observed aberration was found to be of the order of 15 per cent of the cube weight insofar as Throw 1 in the set was concerned.

One objective of the new experiments was to investigate the aberration and the side force when using heavier cubes of the same size (each edge=16 mm.). But in addition to this it was of interest to see whether the degree of roughness of the cube surfaces would affect the results. There are indications in earlier experimental series of such a possibility.

The cubes were made of bakelite, and they had a weight of 5.6 grams each as compared with 2.0 grams for the wooden cubes. Their surfaces were of three different types: (1) the original surface, which is rather smooth; (2) the original surface provided with a network of small depressions; and (3) the original surface covered with glue mixed with fine sand. The three surfaces are designated in the tables as *smooth* (sb), *rough-1* (rlb), and *rough-2* (r2b).

For comparative purposes the releases were made with three smooth cubes and three rough-1 cubes, or with three smooth cubes and three rough-2 cubes; i.e., each release was made with six cubes in one or the other of these two combinations.

Table 1 contains basic data useful for the appraisal of the results which will be used later in the paper in the calculation of the side force acting on the dice. In this table the mean travel distance x_o is the average of 60 measurements. They were carried out with releases composed of three smooth cubes + three rough-1 cubes and 3 smooth cubes + 3 rough-2 cubes, respectively. In the first case x_o for the smooth cubes was 46.67 cm., and in the second case, 46.70 cm. The small difference between these values suggests that the mechanical interaction between the two kinds of cubes released simultaneously is negligible.

The mean travel time t_o was measured for the wooden cubes as .50 seconds, and for the other kinds of cubes it was calculated on the basis of t_o for the wooden ones. This was done on the asumption that the velocity u_o, when the cubes enter the horizontal plane, is the same in all cases. This is a plausible assumption, because the cubes fall a distance in free air after having left the container, and

Table 1

BASIC DATA FOR 16 MM. CUBES

Material	Weight (grams)	Surface	Mean Travel Distance x_o cm.	Mean Travel Time t_o sec.	Friction Coefficient
Wood	2.0	–	46.5	.50	.47
Bakelite	5.6	Smooth	46.7	.50	.38
Bakelite	5.6	Rough-1	48.4	.52	.39
Bakelite	5.6	Rough-2	53.1	.57	.49

the air resistance must be very near the same for all cube types at the small velocities involved.

The velocity u_o for wooden cubes is:

$$(1) \qquad u_o = \frac{2x_o}{t_o} = 2 \cdot \frac{46.5}{.50} = 186 \frac{\text{cm.}}{\text{sec.}}$$

This gives:

$$(2) \qquad t_o = \frac{2x_o}{u_o} = \frac{x_o}{93} \text{ sec.}$$

The friction coefficient is measured for each type of surface. It represents the average of 30 measurements, taken on a plane with the same surface as that used in the experimental apparatus.

RESULTS

Table 2 contains the B-A differences in average cube position in the two parts of this research with bakelite cubes.

Part I of the experiments (Series S9 and S17) was carried out with three rough-1 + three smooth cubes per release. It contains a total of 600 placement throws, 300 of each kind of cube. There were no significant results in this part of the experiment. In addition to the 600 placement throws there were 600 "control" throws (Series S10 and S18), not shown in Table 2, that gave only chance results. They were "controls" in the sense that they were released without any conscious effort to influence their placement.

In Part II (Series S19, S22, S24, and S25) each release comprised three rough-2 + three smooth cubes. In this part, 1200 placement throws (600 of each type of cube) were carried out. The rough-2 cubes, in 600 placement throws, gave a difference of +2.45 cm. between the averages for the *A*- and *B*-targets, with a t of the difference equal to 2.46, which is significant ($P = .014$). As in the

series with wooden cubes reported earlier, there are outstanding results in Throw 1 in the five-throw unit. The *B-A* difference for 120 first-throw trials in Part II is + 7.00 cm., with a *t* of the difference equal to 3.64 (P = .00027).

Table 2

RESULTS IN THE BAKELITE SERIES IN TERMS OF B-A DIFFERENCES

A. Total Trials Within Sets

Placement Series	Cubes Compared	Rough Cubes				Smooth Cubes			
		Trials	Diff.	t of Diff.	P	Trials	Diff.	t of Diff.	P
S9+S17	r1b+sb	300	− .51	....	....	300	− .99		
S19+S22+ S24+S25	r2b+sb	600	+2.45	2.46	.014	600	− .48		

B. Throw 1 Within Each 5-Throw Unit

S9+S17	r1b+sb	60	− .33	....	....	60	−1.67		
S19+S22+ S24+S25	r2b+sb	120	+7.00	3.64	.00027	120	−3.67	1.86	.06

The smooth cubes which were released together with the rough ones in Part II show results at the chance level as far as the total of 600 throws is concerned. There is a slight indication that PK may have operated in Throw 1 on the smooth cubes. In this throw they show a negative difference of −3.67 cm. between averages (*t* of diff. = 1.86; P = .06). This would suggest that they were influenced in the direction opposite to the effect on the rough-2 cubes. As there is some evidence bearing on this hypothesis (especially in Series S22 dealt with later) a Student's *t* test was applied to the difference in the results for the two kinds of cubes for *A*- and *B*-targets separately. The results of this test are contained in Table 3. According to the averages the smooth cubes tended to avoid the target for both *A* and *B* throws. The difference between rough and smooth cubes is pronounced and highly significant when *A* was the target (*t* of diff. = 4.06; P = .00005) as well as for *A* and *B* targets combined (*t* = 3.86; P = .00011). The results of Part II are shown graphically in Fig. 1 for the five successive throws with

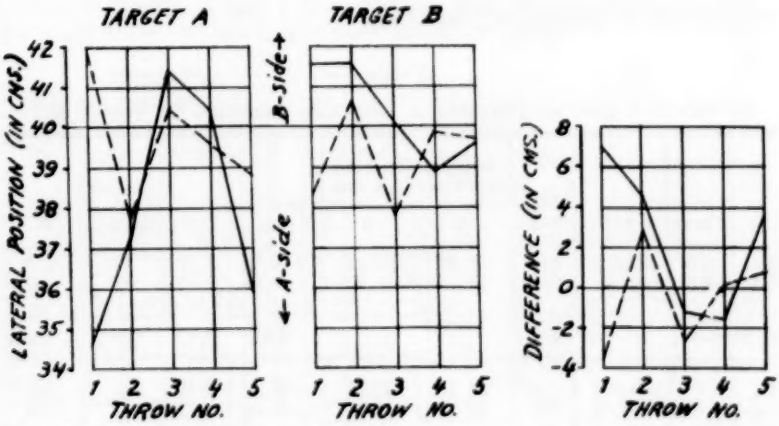
each area as target and in terms of the differences for rough and smooth cubes in each of the five throwing positions.

Table 3

STUDENT'S *t*-TEST OF DIFFERENCE BETWEEN AVERAGES FOR ROUGH-2 CUBES AND SMOOTH CUBES IN THROW 1, SERIES S19+S22+S24+S25*

Target	No. of Trials	Average Position in cms.		Difference in cm.	t of Diff.	Degrees of Freedom	P
		Rough-2	Smooth				
A.....	120	34.58	41.98	+7.40	4.06	59	.00005
B.....	120	41.58	38.32	+3.26	1.59	59	.11
Total....	240	.....	.....	+5.33	3.86	119	.00011

*The data in this table provide a test of the hypothesis that the rough cubes have gone to the target side, but the smooth cubes (which were thrown simultaneously) to the non-target side.



Graphs showing average position of cubes on each of five throws.

Graph showing difference between A- and B-averages in each of the five throw positions.

FIG. 1. Results in Series S19+S22+S24+S25. The solid line represents the rough cubes and the broken line, the smooth cubes.

Among the four series in Part II there is one, namely S22, which should be dealt with separately because the effects were particularly strong in this series and there were interesting trends in the results. Because of a psychological difference in the experimental situation, a distinction has to be made between the first four sets in this series and the last one. It was obvious from mere observation as this

series was going on that the rough cubes favored the target but that the smooth ones disfavored it. As usual I did not summarize the results as the series was in progress, but after having terminated Set 4, I was anxious to investigate the effect mentioned above, and I summarized the results at this point. My impressions formed during the course of the experiments were confirmed: the two kinds of cubes had to a significant degree gone to opposite sides.

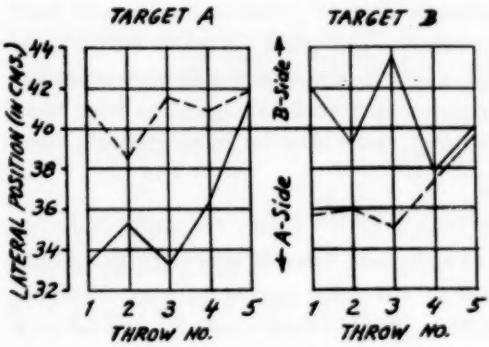
The situation at that point was that I had reassurance about the occurrence of the effect. Naturally I hoped to see the effect continue in the concluding set, No. 5. It did so in Throw 1, for the *A*-target and for the *B*-target as well. But in the Throws 2 to 5 the trend in the results was opposite to that in Sets 1 to 4. The results are illustrated in Table 4 and in Figures 2 and 3. The aberrations are so large that, despite the limited number of trials, the effects are almost consistently of significant magnitude.

Table 4

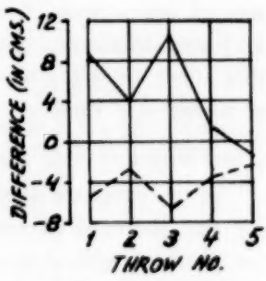
STUDENT'S *t* TEST OF DIFFERENCE BETWEEN AVERAGES IN SERIES S22

Set	Throw	Surface	No. of Trials	Average Position for Target in cms. A	Average Position for Target in cms. B	Diff. in cms.	t of Diff.	Degrees of Freedom	P
1-4..	1-5	rough-2	120	35.85	40.62	+ 4.77	2.10	59	.036
1-4..	1-5	smooth	120	40.87	36.82	− 4.05	2.03	59	.042
Difference..		r2b−sb*	240	.....	.....	4.41	2.92	119	.0035
5....	1	rough-2	6	26.33	52.67	+26.34	6.93	2	.02
5....	1	smooth	6	50.33	33.33	−17.00	1.82	2	.2
Difference..		r2b−sb*	12	.....	.....	21.67	4.53	5	<.01
5....	2-5	rough-2	24	40.17	35.42	− 4.75			
5....	2-5	smooth	24	35.33	48.33	+13.00	3.19	11	<.01
Difference..		r2b−sb*	48	.....	.....	8.87	2.42	23	.025
1-5..	1	rough-2	30	31.80	44.20	+12.40	3.60	14	.003
1-5..	1	smooth	30	43.00	35.27	− 7.73	1.62	14	.13
Difference..		r2b−sb*	60	.....	.....	10.07	3.46	29	.0006

*"Difference r2b−sb" provides a test of the hypothesis that the two kinds of cube have gone to opposite sides within the throws as indicated.

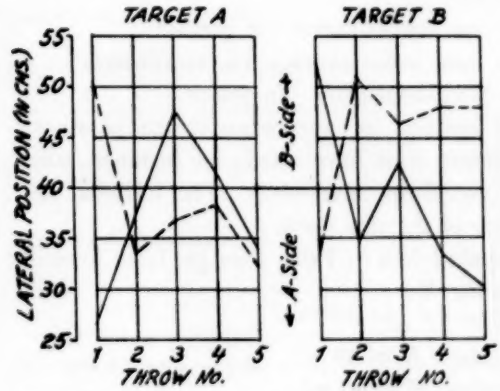


Graphs showing average position of cubes on each of five throws.

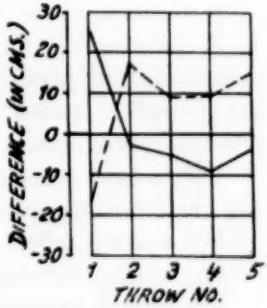


Graph showing difference between A- and B-averages in each of the five throw positions.

FIG. 2. Results in Sets 1 to 4 of Series S22. The solid line represents the rough cubes and the broken line, the smooth cubes.



Graphs showing average position of cubes on each of five throws.



Graph showing difference between A- and B-averages in each of the five throw positions.

FIG. 3. Results in Set 5 of Series S22. The solid line represents the rough cubes and the broken line, the smooth cubes.

An interesting question arises in this connection. Why did the tendency not change as soon as I discovered the effect when the Sets 1 to 4 were still going on? I had a general impression of the effect from observation rather early in the series. But only after having obtained confirmation by specific figures in summarizing the data

of Sets 1 to 4 did the tendency change. Similar experiences have occurred in previous experiments. The point therefore seems to be one of importance, though it obviously requires further attention and confirmation in experiments specifically designed to bear upon this effect since the observations made thus far have generally been of a *post hoc* kind.

Comparison of Side Force with Wooden Cubes and Bakelite Cubes

In the previous work (1) the side force K necessary to account for the observed aberration of the cube aggregate was calculated for Throw 1 with A as target in the placement series S1 and S3 with wooden cubes. K is given by the formula:

(3)

$$K = 4m \frac{y_o'}{t_o^2}$$

where

K = side force (in dynes)

m = mass of dice for one side as target (in grams)

y_o' = aberration from total series average (in centimeters)

t_o = running time on horizontal plane (in seconds)

A statement of K is justified in cases when it can be shown statistically that psychokinesis must have acted; for instance, when the aberration reaches a significant magnitude. The formula then gives an approximate value of the side force.

On the basis of the physical data in Table 1 we get from formula (3) the following values for K:

For wooden cubes: $K = 32\ y_o'$

For rough-2 bakelite cubes: $K = 69\ y_o'$

For smooth bakelite cubes: $K = 90\ y_o'$

where y_o' stands for the aberration of the cube aggregate in each case derived from the over-all averages in the series concerned.

In Table 5 a comparison of the side force in Throw 1 is carried out for A- and B-targets separately for Series S1 with wooden cubes and Series S19 + S22 + S24 + S25 with both smooth and rough bakelite cubes. Justification for a comparison based only on Throw 1 is provided by the low P-values obtained for the B-A differences between averages for these trials as shown in Tables 2 and 3. In other words, these data provide evidence of the strongest PK effects, which we are now interested in measuring.

Table 5

SIDE FORCE *K* AS CALCULATED ON THE BASIS OF THE OBSERVED ABERRATION IN THROW 1 OF THE FIVE-THROW UNIT

	Series S1 Wood Cubes	Series S19+S22+S24+S25 Bakelite Cubes		
		Rough-2	Smooth	Average
Cube weight (grams)	2.0	5.6	5.6	
Friction coefficient	.47	.49	.38	
Series average (cms.)	38.53	39.16	39.50	
Series average with Throw 1, *A*-target, excluded*	(39.46)	(39.67)	(39.22)	
Target A				
Trials	66	60	60	
Average (cms.)	30.11	34.59	42.00	
Aberration (cms.)	+8.42 (+9.35)	+4.57 (+5.08)	−2.50 (−2.78)	
Side force *K* (dynes per cube)	270 (300)	315 (350)	225 (250)	270 (300)
Target B				
Trials	66	60	60	
Average (cms.)	42.67	41.58	38.32	
Aberration (cms.)	+4.14 (+3.21)	+2.42 (+1.91)	−1.18 (− .90)	
Side force *K* (dynes per cube)	132 (102)	167 (132)	105 (80)	136 (106)
Both Targets (average)				
Side force *K* (dynes per cube)	201 (201)	241 (241)	165 (165)	203 (203)

*See text, pp. 150-51.

The choice of Series S1 with wooden cubes for the comparison needs some explanation. As we are here concerned with an attempt to investigate physical conditions and we do not know in advance how different factors may influence the results, it is safe to hold to the established method of experimentation of not bringing in more variables than absolutely necessary. For this reason it was found advisable in comparing the effects on wood and bakelite to consider only the wooden series S1, because only in this series among those

using wooden cubes was the apparatus located in the same position relative to the experimenter as in the bakelite series S19 + S22 + S24 + S25 (namely on a table, elevated from the cement floor). In the later series carried out with wooden cubes the apparatus stood directly on the floor.

In the bakelite series the greater part of the side force in the release—about 60 percent of the total force, both for the *A* and the *B* target—has been used to place the rough cubes. It cannot be definitely said whether this unequal distribution for the rough and smooth cubes is due to the difference in roughness of the cube surfaces, or whether we are confronted with a psychological effect. It is important, however, that the force distribution is practically the same both for *A* and for *B* as target. It seems difficult to assume the high degree of constancy in the psychological control of the forces which would be necessary to obtain such results. We further have to consider that the *K*-values show a consistent relationship to the roughness: the smooth cubes have given the lowest *K*-values; the rough cubes the highest; and wooden cubes (with medium roughness), intermediate *K*-values. The data therefore can be said to give some support to the opinion that the roughness of the cube surface has influenced the aberration.

The *K*-values in Table 5 disclose the interesting point that the total side force in the release of six cubes was almost exactly the same when the cubes were bakelite (the average for rough and smooth) as when they were wood. And this is the case both for *A*- and *B*-targets. The side force for *B* as target is consistently nearly half as large as that for *A* as target.

Discussion

The general opinion nowadays seems to be that ESP and PK have nothing to do with physics. The circumstance that the results in ESP experiments show no dependence upon distance is considered as lending support to such a hypothesis; and in dice experiments for target faces, variation in the weight and size of the cubes has not given significant differences. This latter observation has strengthened the opinion held by some investigators that very small forces may be acting on the cubes at strategical instants. A crucial test of the hypothesis whether psi is independent of the physical conditions

has not, to my knowledge, been carried out so far. But the opinion has been advanced that such a test would be very difficult, if not impossible, to devise because the psychological conditions in the test situation could not be quantitatively specified. It would therefore at least be very difficult, it is argued, to distinguish between the influence of physical and psychological conditions in a psi experiment.

This, however, should not be a reason to restrain from investigations intended to solve this important question. The psychological factors are not entirely unknown. Even if we cannot specify how they act in a single trial, we are by now at least to some degree informed about their influence in an extended series of trials. And this helps toward forming a judgment concerning a possible influence of physical conditions. In this connection attention may be directed to the well known decline or U-distribution of hits in an experimental unit, and to the related outstanding effects in the first trial of such a unit.

It is difficult to see how the question about the influence of physical conditions could be investigated in any other experiment than one dealing with psychokinesis. Physics always requires a *force* for the production of a movement, or for a change in the movement of an object. The study of the influence of physical factors on the PK effect therefore involves the investigation of *forces.* The usual dice experiments for faces do not lend themselves to dynamical investigations. The conclusions drawn from such experiments with respect to the influence of cube weight, size, etc. are therefore questionable. But we have now in the scaled placement method a means of dealing with the dynamics of a moving cube aggregate, and this method therefore seems promising for solving the problem of the influence of physical factors. But in dealing with the problem we will have to consider the possible psychological influences as we know them from earlier experimentation.

If we consider in this light the bakelite series in which three rough-2 and three smooth cubes were released simultaneously, we find once more a distinct U-curve tendency in the five-throw unit for the *rough* cubes, with the maximum aberration in Throw 1. (See Figure 1.) But the *smooth* cubes, which were supposed to veer off to the same side as the rough ones—and conscious attempts were made to achieve this—show a different aberration pattern.

According to Fig. 1 it looks as if they were subjected to an influence in the direction opposite to that of the rough cubes, as far as Throw 1 is concerned ($P = .00027$ for the rough cubes, and $P = .06$ for the smooth ones in this throw), whereas the aberration tendency for the smooth cubes in Throws 2 to 5 is on the whole the same as for the rough cubes. It appears that in Throw 1 for the smooth cubes we may be confronted with a case of psi-missing.

The highly significant difference between the averages for rough and smooth cubes in the first throw ($P = .00011$) justifies a closer examination of these results and a comparison of the side force K necessary to account for the aberrations in this throw. Table 5 shows an evaluation of K that was carried out on the basis of the total averages of the two kinds of cube in Part II, considering the first throws for A- and B-targets separately. Figures for the wooden cube series S1 are given in the table for comparative purposes.

It is seen in the table that the negative deviation of the smooth bakelite cubes in Throw 1 originates both from A- and B-target throws, the aberration for A-target being about twice that for the B-target. About the same relation is found between the positive deviations for the rough cubes, and for the wooden cubes as well. The side force K necessary to produce the aberration when the A-side was the target thus is consistently about twice that for the B-side target.

It is interesting to compare the average force per cube in the bakelite series with that in the wooden series. As seen in Table 5, the K-values for bakelite and for wood are almost exactly the same, both for the A- and the B-target (270 and 132 dynes for wood, as compared with 270 and 136 dynes for bakelite). These results strongly suggest a connection between the conscious efforts exerted by the subject in the experiments and the aberrating force. They are consistent with the hypothesis that the magnitude of the PK forces depends on the strength of the conscious efforts, but that the *control* of the forces is a special matter which has both conscious and unconscious aspects. In the present case the positive aberration of the rough cubes is considered to be due to conscious control, but the negative aberration of the smooth cubes to an unconscious control.

The fact that opposite aberrations for rough and smooth cubes were arrived at in the same throw discloses once again the *selective* character of the PK mechanism. This has been previously demonstrated, more particularly in a series with paper cubes and wooden cubes thrown simultaneously (2), in which the subject was trying to influence only the wooden cubes.

* * * * *

We will now turn to the main problem in this study; namely, that of *the possible influence of the physical conditions of the experiment on the placement results.* Table 5 gives data for the following *independent physical variables* considered in the experimental series: the cube material, the cube weight, and the cube surface. In selecting the wooden cubes in Series S1 for the comparison we have chosen data in which the subject was always in the same location relative to the experimental set-up. He stood on the *A*-side of the apparatus and at a distance of about one meter from the table when releasing the cubes (the surface of the dice table was 87 cm. above the floor level). The fact that the subject had his place on the *A*-side in releases both for *A* and *B* as targets necessitates the making of a distinction in the results for the two target areas. This is a further independent physical variable to take into account when appraising the results.

We do not know at present whether the cube material itself is a factor in PK experiments. However, in this investigation we may ignore the differences in material (wood *versus* bakelite) and consider only the cube weight and the cube surface because the material itself is not a factor in a mechano-dynamical calculation. The cubes have all the same dimensions (side = 16 mm.). Edges and corners are but slightly rounded (radius less than one millimeter).

The cube weight is a well-defined property. Concerning the surface we are only interested in its roughness (and not, for instance, in its color), because we desire a feature of a *mechanical* nature for the discussion of a possible relationship between the surface conditions and the calculated side forces.

As a measure of the roughness, we introduced earlier in this report the friction coefficient as measured on the *dice table surface,* which is rather smooth. This figure, naturally, is somewhat arbi-

trary. Other values are obtained when taking the measurements on a plane with a different degree of smoothness. However, we are here primarily interested in getting a general idea of the comparative smoothness of the cubes. Comparative measurements on a rough plane have given values with the same relative frictional tendency. The three kinds of surface on the cubes therefore can safely be arranged in the following order with regard to their smoothness: smooth bakelite, wood, rough-2 bakelite.

The dependent physical variables consist of the aberration y_o' and the running time t_o, which are both obtained from measurements. The comparative measure for the PK effect is the side force K necessary to account for the aberration. And K is calculated on the basis of y_o', t_o, and the cube weight.

We now have to try to get a picture about the influence of the psychological conditions. In these placement experiments we have repeatedly found a U-distribution of hits among the five successive throws for one area as target. In Throw 1 for A as target the aberration has consistently been very high, in the positive direction. This is the general trend in series when the subject made strong conscious efforts to achieve results. In so-called "control" series, when the subject took a neutral attitude, the trend is the opposite; i.e., there is a reversal of the U-curve in the five-throw unit, and the deviation in Throw 1 is as a rule negative. These circumstances give a strong indication that the conscious efforts exerted by the subject play an important part in producing the PK effect.

In previous reports (**2**, **3**) special attention has been given to the U-distribution of hits. It has been shown statistically that the U-curve approaches a parabola to a highly significant degree. Of special importance is the fact that the fit to the parabola improves with the number of trials (**2**). The close fit to a mathematical curve such as a parabola cannot easily be attributed to random variation. There must be a reason for it. And the nearest explanation is that a high degree of consistency prevails in the subject's scheme of efforts in the five-throw unit as the experimental series proceeds.

From this point of view it must also be assumed that the efforts exerted in Throw 1 of the unit are rather constant when taken as an average in an extended series. From the comparative treatment of

the data obtained in the actual wooden and bakelite series it therefore would appear that there is justification for claiming the occurrence of PK forces of the same magnitude with wooden cubes and with bakelite cubes. We may take this as a plausible working hypothesis when discussing the results contained in Table 5.

A set of figures has been added in this table in parentheses. These figures are arrived at when the first throw for *A* as target is excluded. The high aberrations in the first throw have the largest influence on the average among the ten throws in the unit, and it was therefore advisable to investigate whether the exclusion of Throw 1 for *A* would substantially affect the general comparison of the three kinds of cubes. This is found not to be the case in the data as a whole, though there is a noteworthy difference in the relationship between the figures for *A*- and *B*-targets. The *K*-values for *A* as target are somewhat increased, and those for *B* as target are reduced by the same amount.

Each release comprised six cubes, both in the wooden series and in the bakelite series. In the wooden series the cubes were all of the same kind, but in the bakelite series three of the cubes had a rough surface and three cubes had a smooth surface. Of the six bakelite cubes released simultaneously, the rough ones went to the target side, the smooth ones went to the non-target side. This different behavior of the two kinds of bakelite cubes appears to be a psychological matter. It is of minor importance for the dynamical discussion because the force necessary to produce a certain aberration does not depend on the sign of the aberration.

According to Table 5 we get in the bakelite series a mean side force *K* per cube of almost exactly the same value as in the wooden series, both for *A* and for *B* as target. This means that the average side force per cube was substantially the same in the two series, as is seen in Table 6.

In this table the figures in parentheses refer to the series average excluding Throw 1 for *A*.

This result is in full agreement with the psychological working hypothesis, and it indicates that the cube weight is a factor: a heavier cube is subject to a smaller aberration than a lighter cube. The circumstance that this applies both to *A*- and *B*-target throws adds to the weight of the finding.

Table 6

APPROXIMATE AVERAGE SIDE FORCE EXERTED UPON EACH CUBE IN THROW 1 FOR BOTH WOOD AND BAKELITE CUBES*

Target	SIDE FORCE PER RELEASE					
	Wood Cubes			Bakelite Cubes		
A.....	270	(300)	dynes	270	(300)	dynes
B.....	132	(102)	dynes	136	(106)	dynes
Average.....	201	(201)	dynes	203	(203)	dynes

*The values in parentheses are the results when the series average is obtained without Throw 1 for the *A*-target.

In comparing the side force for targets *A* and *B*, we also find a high degree of constancy in the conditions. This is seen in Table 7, showing the percentage of distribution of the side force *K* on *A*- and *B*-targets.

Table 7

PERCENTAGE DISTRIBUTION BETWEEN *A*- AND *B*-TARGETS OF SIDE FORCES MEASURED IN THE FIRST THROW OF THE FIVE-THROW UNIT FOR THREE KINDS OF CUBES*

Target	PERCENTAGE OF SIDE FORCE FOR THROW 1			
	Wood Cubes	Bakelite Cubes		
		Rough	Smooth	Average
A.....	67.2 (74.7)	65.4 (72.7)	68.2 (75.7)	66.8 (74.2)
B.....	32.8 (25.3)	34.6 (27.3)	31.8 (24.3)	33.2 (25.8)

*The values in parentheses are the results when the series average is obtained without Throw 1 for the *A*-target.

The side force for *A* as target thus is consistently very near twice the value found for *B* as target, or three times this value if we base the comparison on a series average calculated with exclusion of Throw 1 for *A*.

This unequal distribution indicates a relationship to the location of the subject during the experiment. He stood consistently on the *A*-side of the apparatus. However, it is difficult to decide whether the effect is to be attributed to psychological or to physical conditions. Psychologically the subject may have felt it easier to produce a "pulling" effect on the cubes than a "pushing" one, and this may

have governed his efforts. But the possibility should not be overlooked that the matter can have a physical aspect. The differences in the K-values for the two targets are so great that it seems questionable that they can be attributed only to variation in the subject's efforts.

* * * * * *

The above discussion pays attention only to the results in Throw 1 of the five-throw unit where the greatest and most consistent aberrations were obtained. Only in this throw do we have the distinct oppositional behavior of the smooth and the rough cubes when considering the four bakelite series of Part II in their entirety. But we learn from the results in Series S22 (Table 4 and Figures 2 and 3) that oppositional aberrations may occur also in the remaining throws. This circumstance brings up the question: Why do we find this opposition as a rule only in Throw 1?

We shall deal with this question in connection with a hypothesis which lends itself to consideration on the basis of the test material.

It was seen from the graphs in Figures 2 and 3 that the oppositional effect is very pronounced and consistent in Series S22. The K-values are high. Thus if we consider, for instance, the first three throws in Sets 1 to 4 of this one series, we arrive at an average of 270 dynes for the rough cubes, and 220 dynes for the smooth ones, as compared with the figures 241 and 165 dynes for Throw 1 in the aggregate of the four series. The occurrence of oppositional aberrations with the two kinds of cubes thus seems to be linked up with the occurrence of *high* PK forces. This circumstance gives weight to the opinion that a necessary condition for obtaining a large aberration with one kind of cube is that the other kind should simultaneously veer off in the opposite direction.

But this involves an aspect of an interesting nature; namely, that there must be a physical interaction between the two kinds of cubes when they move on the plane. Such an interaction may be more likely to occur when the subject makes strong efforts to achieve results, and this may be the reason why the effect is generally observed only in Throw 1 in the five-throw unit, where pronounced results are consistently arrived at.

The hypothesis offered here seems to be in need of serious consideration in further placement PK research work.

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ESP TESTS WITH A SPANISH GIRL

ABSTRACT: The discovery of high-scoring ESP capacity in an adolescent Spanish kitchen maid is the theme of the introductory section by John and Patricia Langdon-Davies. Mr. and Mrs. Langdon-Davies first tested the girl under informal exploratory conditions and found her capable of producing between two and three times as many hits as would be expected from chance alone. Even when the test cards were shielded by cardboard she continued to get approximately twice the expected score. Both in the tests of clairvoyance and in the GESP condition (which allows for both telepathy and clairvoyance) about the same scoring average was obtained. However, when Mr. Bateman and Dr. Soal took over the experiments (as described in the second section of the report), the clairvoyance scores dropped to the chance level. (Dr. Soal's earlier work, it will be recalled, also characteristically showed only chance scoring in clairvoyance.) In the GESP tests Maria continued to score above chance, giving significant results even though at a lower scoring level than she had shown for Mr. and Mrs. Langdon-Davies. —Ed.

[Through the courtesy of the Parapsychology Foundation we are able to present here the original report of Mr. F. Bateman and Dr. S. G. Soal on the experiments conducted in Spain with the subject, Maria, recently discovered by Mr. and Mrs. Langdon-Davies to be a high-scoring ESP subject. In order to introduce this report more adequately to our readers, we are reprinting (with permission) the following letter from Mr. and Mrs. Langdon-Davies which appeared in the September, 1955, number of the *Journal of the Society for Psychical Research* under the title "ESP Experiments with Maria" (pp. 146-49).—Ed.]

I. The Experiments by Mr. and Mrs. Langdon-Davies

In view of the publication of an unsigned note in the *Journal* with regard to Maria, the Spanish card-sensitive who has been investigated by my wife and myself, a brief explanation seems called for from her investigators.

We cannot at the present time publish a scientific paper on our researches because in December the Ciba Foundation will be publishing the paper read by one of us to the Foundation's recent Conference on ESP. After that publication a full report will be available. In the meanwhile the following are the bare facts:

In November last my wife, acting on a sudden whim, resolved to test three Spanish servants with a pack of Zener [ESP] cards. The first run of each girl in turn produced 16, 16, and 15 correct guesses.

My wife informed me and Dr. E. J. Dingwall, who was staying with us, and we at once repaired to the kitchen. We there observed the three girls consistently guessing an average of over 11 correct guesses per run of 25 cards. The conditions were in no sense rigorous as we neither had the resources for creating laboratory conditions nor were we expecting the very sensational results. However, the three observers were fully conversant with the main types of sensory leakage to be expected and were unable to detect any such leakage.

The final figures with the three girls acting as agent and percipient in turn were as follows:

Runs	*Correct Guesses*	*Expected*	*Average*	*CR*
76	855	380	11.2	27

Domestic considerations prevented any further testing (to date) of two of the girls, but Maria fortunately remained with us.

In the course of the following weeks more satisfactory experimental techniques were instituted. The cards were enclosed in opaque covers and manipulated in a manner which ruled out any information reaching the percipient from back or face. There still remained the possibility that sensory information was being carried from agent to percipient by glances at the correct key card since no screen was being used. It is therefore of the utmost importance to observe that during one fairly long series "clairvoyant" and "telepathic" methods were alternated.

The "clairvoyant" method was to place the pack, unseen after shuffling by the agent, on the table between the agent and Maria and to have her match card by card (each in its opaque cover) with the key cards.

In this series the "clairvoyant" runs actually produced *more* correct guesses than the telepathic. This result suggests that whatever the explanation it could not be sensory leakage from the agent glancing at the correct key cards. The average correct guesses per run in this series of "clairvoyant" results was 10.0 and for the corresponding "telepathy" runs 9.9.

The total results for all runs with cards covered *and ignoring the first spectacular results with bare cards* were:

Runs	*Correct Guesses*	*Expected*	*CR*
810	5848	4050	32 (approx.)

Towards the end of the series, Maria showed signs of boredom, and domestic circumstances were such as might be expected to upset the experimental *gestalt*. However, her final scores began to rise after a temporary fall to chance level and, judging by previous results, would have continued to rise had the experiments been continued. We were interrupted by preparations for a two months' visit to England.

On our return a new experiment was begun. Maria remained at chance level for some time but gradually worked up a CR of 4.1. The last stages of this experiment were observed by Dr. Soal and Mr. Bateman who through the good will of Mrs. Eileen Garrett were able to come out and carry through a brief confirmatory investigation.

They at once pointed out, what we would never have disputed, that the experiments without a screen laid themselves open to the criticism that sensory leakage might take place from the agent, but after observing a number of runs they agreed that they were unable to find any evidence that this was so. They put this to the test by carrying out an experiment with Mr. Bateman as agent concealed from Maria by a screen. Once more Maria began at chance level and it was not unlikely that she was overawed by the attentions of two important gentlemen from abroad. (She is a 16-year-old housemaid.) Eventually her guesses began to improve in a way which was astonishingly similar to that in which they had improved in the previous experiment to test which this one had been designed.

After comparing the two results, Dr. Soal and Mr. Bateman agreed that the type of sensory leakage ruled out by the Bateman experiment was very unlikely to be the explanation of the similar result in the other experiment. Thus this evidence, added to the evidence afforded by the earlier "clairvoyant" results, suggest that the whole series of results of testing Maria are worthy of the very closest attention.

While very properly not endorsing any part of the experiments which he had not seen himself, Dr. Soal summed up his impressions of Maria in words which he wrote in our copy of Soal and Bateman: "During the past ten days I have witnessed a statistical demonstration of the powers of the third card-guessing sensitive that it has been my privilege to meet."

This makes Maria important enough perhaps, but we cannot help regarding the original testing of the three servant girls, with all its obvious technical shortcomings, as being even more important, and we are as prepared to accept the necessity of invoking ESP to explain this unique result as we are to invoke it for the experiment with Maria supervised by Dr. Soal.

Directly domestic circumstances make it possible, a long series of further experiments will be begun. Their objective will not be to "prove" ESP once more, but to examine the *modus operandi*. It is hoped that more than one card-sensitive will be unearthed besides Maria herself, and we shall be glad to carry out any experiment for those whose study of ESP is complicated by lack of human guinea-pigs. Any further experimentation will be carried out in accordance with the requirements of rigorous laboratory technique. We shall be particularly glad of suggestions for new forms of experiment.

Our thanks are due to the Ciba Foundation for permitting us to publish this preliminary report in the *Journal* and to Mrs. Eileen Garrett, Dr. S. G. Soal, Mr. F. Bateman and Dr. E. J. Dingwall for their interest in our work.

II. The Report of Mr. F. Bateman and Dr. S. G. Soal[1]

We reached San Feliu on the morning of April 16. On our arrival Mr. Langdon-Davies showed us the scores which he had obtained with his 15½ year old subject *Maria* during the seven days preceding our visit (i.e., between April 7 and April 15). While these scores were definitely significant, the rate of scoring was only about 5.5 hits per 25 trials on the average. Clearly there had been a very considerable decline in Maria's powers since the phenomenal scoring of last November and December. It was interesting to note that only one of his 15 sheets of 200 guesses showed a score which was less than 40, the chance expectation, while three sheets had scores of 50 or more.

After watching Mr. Langdon-Davies' experimental method, which did not make use of any screen but in which the cards, when presented to the subject, were covered by cardboard folders, we came to the conclusion that there was no possibility of Maria's catching

[1] This investigation was made possible by a travel grant from the Parapsychology Foundation of New York.

a glimpse of the cards inside the folders. Nor, if Mr. Langdon-Davies' account is correct, was it possible for Maria to recognize the card inside any folder, since he told us that she never saw the checking or had access to the folders at any time in order to study them for specks, etc. Since, however, Maria indicated her guess by pointing to one of the five target cards laid in a row in front of her, there appeared to be a possibility that as Mr. Langdon-Davies looked at each card inside its folder, he might immediately afterwards glance at the corresponding target card and that Maria might follow the motion of his eyes or of his head.

We began our own experiments on the afternoon of April 16. S. G. S. had brought with him from England 16 packs of cards bearing colored pictures of five animals, and these, after randomization by means of Tippett's tables, were placed individually in 400 opaque envelopes into which they exactly fitted. The randomized cards were made up into 16 packs of 25. The flaps of the envelopes were turned down but not sealed. Before each experiment of 400 trials a fresh randomization of the envelopes was made and the corresponding card order was written out in the target columns of the scoring sheets before the experiment. These scoring sheets provided space for 200 guesses on each sheet.

Before each run of 25, the cards in their envelopes were placed in a pile and the envelopes were lifted off the pile one by one and presented to Maria with the flaps on the underside. Maria indicated her choice by placing the card opposite one of the five target cards laid face upwards in a row on the table. F. B. recorded her choice. The cards were not removed from their envelopes during the checking, which was done with the help of the prepared target lists.

On April 16 and 17 a total of 1200 trials was recorded under these *clairvoyance* conditions, but the total of 243 hits achieved by Maria was only 3 above chance expectation (240) and was not significant. The scores are shown in Table 1.

Maria was rather upset by this failure under the clairvoyance condition, and we deemed it advisable to give her a day's rest. On the morning of April 19 we began a fresh series in which the telepathic condition was permitted to operate, with F. B. as the agent. In the meantime F. B. had made friends with Maria who gave him

Table 1

RESULTS OF CLAIRVOYANCE TESTS

Date	Time	Trials	Hits	Dev.
April 16	Afternoon	200	36	−4
April 16	Afternoon	200	46	+6
April 17	Morning	200	37	−3
April 17	Morning	200	48	+8
April 17	Evening	200	41	+1
April 17	Evening	200	35	−5
Totals		1200	243	+3

some impromptu instruction in Spanish. We feel that this may have helped us to obtain the significant results which followed.

A wooden screen was rigged up by Mr. Langdon-Davies with a gap of about an inch between the bottom of the screen and the table. Beneath this gap were placed the five animal cards to which Maria was to point with a pencil and so indicate her choice to the recorder (Mrs. Langdon-Davies) sitting on the opposite side of the screen, i.e., on the same side as F. B. The screen itself ought to have been an inch or two higher, since a small tuft of F.B.'s hair was visible to Maria. The rest of him, however, including his eyes, was out of sight. We decided to revert to the packs of cards in individual folders previously employed by Mr. Langdon-Davies. These folders were in 16 packs of 25, there being five cards of each symbol in a pack. Before each experiment, every pack was thoroughly shuffled, and after the shuffle, S. G. S. gave the pack a random cut.

The experiment was carried out at an extremely rapid speed; the guessing of 25 cards occupied a bit more than 40 seconds. F. B. on his side of the screen lifted up each folder, looked at the enclosed card which was attached to the inside of the upper half of the folder, and plumped it audibly on the table. Maria pointed to one of the target cards. At the end of each run of 25, F. B. read out the order of the target cards and Mrs. Langdon-Davies recorded it and counted the number of hits. The bulge of the folder was always towards Maria, and S. G. S. verified that even had she brought her eyes down to the level of the gap, she could not have seen the enclosed cards.

On several occasions Mr. Langdon-Davies, unable to conceal

his eagerness to know how Maria was progressing, walked into the room during the experiment, and it was noticed that when the scoring was high before he entered it fell immediately afterwards to chance level or below. This was unfortunate, and we think that had the subject been left undisturbed, even better results would have been obtained.

Seldom were the scores (out of 200) below 40, and a feature of the guessing was the way in which the score would suddenly leap up at intervals to the fifties. Thus, on the evening of April 19, Maria obtained a score of 58/200 followed by a score of 52/200. This reaches the level of guessing achieved by Mrs. Stewart,[2] but Maria was unable to sustain this high rate.

On a total of 2,400 telepathy trials with F. B. as agent, Maria made 549 hits on the target or "0" card. This represents an average of 5.72 hits per 25, and the critical ratio is 3.5 with odds of 2,150 to 1 against chance. The scores are given in Table 2.

Table 2

TELEPATHY TESTS WITH SCREEN (AGENT: F. BATEMAN; RECORDER: MRS. LANGDON-DAVIES)

Date	Time	Trials	Hits	Dev.
April 19	Morning	200	48	+ 8
April 19	Morning	200	42	+ 2
April 19	Evening	200	58	+18
April 19	Evening	200	52	+12
April 19	Evening	200	37	− 3
April 20	Afternoon	200	41	+ 1
April 20	Afternoon	200	52	+12
April 20	Afternoon	200	43	+ 3
April 20	Evening	200	43	+ 3
April 20	Evening	200	45	+ 5
April 21	Morning	200	49	+ 9
April 21	Morning	200	39	− 1
Totals		2400	549	+69

CR = 3.5
P = .0005

Owing to Maria's being nervously upset by this long spell of guessing, we decided to let her rest on April 22 and to commence another series of telepathy experiments with Mrs. Langdon-Davies as agent and F. B. as recorder on the following day, April 23.

This series unfortunately had to be terminated on the evening

[2] See *Modern Experiments in Telepathy* by S. G. Soal and F. Bateman.

of April 25 owing to our departure early the next morning. The results are given in Table 3.

Table 3

TELEPATHY TESTS WITH SCREEN (AGENT: MRS. LANGDON-DAVIES; RECORDER: F. BATEMAN)

Date	Time	Trials	Hits	Dev.
April 23	Afternoon	200	39	− 1
April 23	Afternoon	200	53	+13
April 23	Afternoon	200	40	0
April 24	Morning	200	47	+ 7
April 24	Morning	200	42	+ 2
April 25	Afternoon	200	50	+10
April 25	Afternoon	200	48	+ 8
April 25	Evening	200	34	− 6
April 25	Evening	200	38	− 2
Totals		1800	391	+31

CR = 1.83
P = .07

When the totals for Tables 2 and 3 are combined, the results are as follows:

Number of Trials	*Number of Hits*	*Deviation*	*CR*	*P*
4200	940	+100	3.86	.00011

The average score for all experiments done under telepathy conditions while we were present was 5.6. It will be seen that the odds work out at 8,000 to 1. Our critical ratio would have been higher than 3.86 but for the below-chance results obtained on the last evening, when Maria was a little excited and perhaps consciously trying too hard.

GENERAL REMARKS

As a result of our visit we consider that further experiments with Maria would be well worth while. S. G. S. has impressed on Mr. Langdon-Davies the importance of using a good screen in all experiments and also the necessity of changing the folders which contain the cards. On no account should these folders be accessible to Maria as she goes about her household tasks, and she should never be allowed to witness the checking, though she should be told her score after each run. It might alternatively be possible to condition Maria to work in a room adjoining that in which the agent is sitting—though this, we consider, is not immediately important. One great

difficulty about Maria is the fact that apparently quite insignificant changes in experimental conditions produce an immediate drop in the score, which may remain at chance level over a series of several hundred guesses before it recovers. For this reason varying the physical and psychological conditions of the experiment is going to prove rather difficult. We have suggested various experiments to Mr. Langdon-Davies such as a protracted series of tests to discover whether she can really succeed with clairvoyance, and if so, whether the telepathic condition is a help to higher scoring. Or again, does she, unlike Mrs. Stewart, achieve a higher score when two agents are looking at the same card than with only a single agent? At present, while there is a definite upward trend in the critical ratio, the scoring is not nearly so consistent as that produced by Mrs. Stewart. Apparently Maria achieves significance by occasional spurts followed by scores only slightly above the level of chance. (She rarely guesses below chance level.) This lack of consistency will make any kind of differential experimentation extremely tedious and exasperating. But given patience and persistence, it should be possible to compare results under different conditions.

Whether Maria will ever recover the phenomenal scoring of the early days, or whether she will even maintain her present rate of scoring, it is impossible to predict. It is our conjecture that she has been overworked by too long and too frequent spells of card-guessing and we have suggested to Mr. Langdon-Davies that she should be given a rest for some weeks, and that in future the sessions should be held not more than once or twice a week and limited to a series of about 400 trials at each sitting. Otherwise we think there is a very real danger that she will lose the psychic power that remains to her.

We have further suggested to Mr. Langdon-Davies that he should make every effort to carry out experiments with other subjects from the south of Spain, and, if possible, to re-examine the other two girls who produced phenomenal scores in Dr. Dingwall's presence.

RELATIONS OF MENTAL SET AND CHANGE OF SKIN RESISTANCE TO ESP SCORE

By Soji Otani

ABSTRACT: This report not only represents a geographic area that is new to parapsychology but it introduces novelties in the experimental procedure as well. One new feature consists of the fact that the author himself participated as the subject, with his students serving as experimental observers. In a clairvoyance card-calling test he made a total of 3700 trials equally divided between two conditions, each condition representing a definite mental set. In one condition he closed his eyes and concentrated on high scoring, and in the other he took an indifferent attitude. Skin resistance was measured before the start of each ESP run and at the end. It will be sufficient here to say that some markedly significant differences were found, not only with regard to mental set but also in relation to the changes of skin resistance indicated by the measurements. There was also a significantly negative backward displacement. The author, however, regards his results as exploratory and draws only very tentative conclusions.

Mr. Otani teaches experimental psychology at Chiba University in Japan. —Ed.

Introduction

It is known that psi functions are affected by various psychological factors which cannot be controlled consciously. Many investigators of psi are trying to find more stable and favorable conditions for psi success. The effect of attitude is one of the psychological problems under investigation. The influence of the subject's opinion of psi upon his scoring in psi tests has been investigated; and more recently, the effect of the subject's personal values on ESP has been studied (**2**, **5**). In addition to this problem, the influence of the subject's mental set is also important; it is believed by many that relaxation and a passive state of mind are conditions favorable to psi. One of the objectives of the experiment reported here is to discover whether two artificially introduced mental sets in a subject would produce different ESP results.

Another aim of this research is a physiological one. The effects of various drugs on psi performance have been investigated and certain relations indicated, but as yet no clear evidence has been found of a relation between measurements of the subject's physiological condition and his psi performance. R. J. Cadoret, in his experiment on drug effects, reported that he did not find any relation

between the electrical effects measured on the subject's body surface and ESP performance (1). At the same time, the palmar skin resistance appears to be a sensitive indicator of physiological conditions; it may be used as an index of postural change and it also shifts its general level with some sedentary activities (4). Therefore, I planned to measure the skin resistance of the subject during ESP tests to see whether there would be any correlation between the change of its general level and the ESP score.

Procedure

The experiment was carried out during the period from March 9 to May 18, 1955, in the Psychological Laboratory at Chiba University, Japan, with the cooperation of three students, Messrs. Kozaki, Nakajima, and Shioya, who understood the purpose of the experiment. The experimenter was one of these students, and the writer himself was the subject.

The clairvoyance BT method was used, with the standard ESP deck. The measurement of skin resistance was made by reading the meter of the psychogalvanometer. The experimenter and the subject were in the same room, with an opaque screen between them, the subject sitting at a table. A tin electrode, one-third of an inch square, was attached to the middle and the fourth fingers of his left hand which either rested on the table or was in a temperature regulating box set to keep the temperature of the skin surface at about 30° C. The subject recorded his calls with his right hand. The average values of temperature and humidity of the room were 18.4° C (maximum 25.0° C; minimum 11.0° C) and 73.5% (maximum 81%; minimum 65%) respectively.

About five minutes after the application of the electric current to the skin, the experiment started off. Each trial was given 10 seconds, because it was necessary to keep the time of each run constant. A signal for each trial was made by the experimenter, and a five minute rest period was inserted between the runs. Checking of hits was not conducted until the session was completed.

There were 37 sessions of four runs each, and one or two sessions were carried out in a day. The experiment was divided into two series, but the second series had to be given up at the half-way point because of an accident to the apparatus. Therefore the first

series and the first half of the second one are reported here together. The subject was asked to take one of the following two mental sets during the test: in one set he closed his eyes and concentrated upon scoring high (called the S_1 condition); in the other he was indifferent toward scoring and did not close his eyes (called the S_2 condition). From the first to the twenty-fifth session, one of these mental sets was held for each run and the order was randomly arranged in each session, with the restriction that there were always two runs for each mental set. From the twenty-sixth to the thirty-seventh sessions, either of the two mental sets was maintained for a whole session, and they were taken from session to session in random order.

Results

The Change of Skin Resistance

In the investigation of the psi function, it is an interesting problem whether there may be a direct relation between ESP score level and the galvanic skin reflex; but it is doubtful that this reflex may have any significance as an ESP index, because it is easily influenced by very slight stimuli, external or internal, which are not related to the ESP test itself. Hence, in this experiment, the change of general level of skin resistance was used as an index, and its value was represented by the difference between the resistances immediately before and immediately after the run. The difference was written with a plus sign in front of it if the second value was larger than the first, otherwise with a minus sign. These values were distributed as shown in Table 1.

Table 1

Distribution of the Changes of Skin Resistance During ESP Run

	Resistance Changes in 1,000 Ohm Units ($k\Omega$)							Average Change
	≤ -10	-9 to 0	$+1$ to $+10$	$+11$ to $+20$	$+21$ to $+30$	$+31 \geq$	Total	
Frequency.....	6	12	56	40	22	12	148	11.8

Though the skin resistance during the ESP run decreased in 18 cases, the general trend was an increase; the average was $+11.8$ k Ω (thousand ohms). After the end of the run, the skin resistance

in most cases decreased rapidly during the rest period. These results are the opposite of Freeman's findings (4); according to him some sedentary activities caused the decline of skin resistance. On the other hand, Conklin reported that the level of conductance of the skin did not show any systematic change when the subject was reading (3). The data from the same subject from a previous experiment indicate that the general rise in skin resistance was not characteristic of this subject's ESP performance; but the tendency had been observed in other activities like reading and finger oscillation. There is no evidence by which to decide whether this trend is peculiar to this subject or not.

The change of resistance level was separated into two groups in accordance with the mental sets. The average change in skin resistance for S_1 was 13.9; for S_2 it was 9.8. The *t* of the difference between the two sets was 2.11 ($P < .05$). When this result is compared with Freeman's and when the mental sets S_1 and S_2 in this experiment are assumed to correspond to his sets, "supernormal effort" and "congenial effort," the results were the reverse of those he found.

ESP Results

The total ESP results are shown in Table 2 for the direct (0) target, forward (+1) displacement, and backward (−1) displacement. The direct target gave a negative deviation with an average of 4.64 hits per run (not significant). This average is very close to that which the subject had in a clairvoyance experiment using the DT procedure, in July, 1951. His score average in that experiment was 4.73. When the results of the two experiments are combined for their suggestive value, the total deviation is negative to a significant degree: $CR = 2.72$; $P = .007$. It is conceivable that this scoring level indicates the basic ESP ability of this subject. There are no significant total deviations from chance expectation on (+1) and (−1) displacement.

ESP Scores As Related to Mental Set and Change of Skin Resistance

When the results were divided into two sections by mental sets S_1 and S_2, a significant negative deviation was obtained in S_2 on (−1) displacement, and the difference between S_1 and S_2 scores on

Table 2

RELATION BETWEEN MENTAL SETS AND ESP SCORES ON (0), (+1), AND (−1) TARGETS

Mental Set	(0)			(+1)			(−1)		
	Trials	Hits	Dev.	Trials	Hits	Dev.	Trials	Hits	Dev.
S_1	1850	342	−28	1776	334	−21.2	1776	373	+17.8 } †
S_2	1850	345	−25	1776	357	+ 1.8	1776	301	−54.2* }
Total	3700	687	−53	3552	691	−19.4	3552	674	−36.4

*CR = 3.21; P = .0013.
†CR_d = 3.02; P = .0025.

(−1) displacement was also significant. (See Table 2.) As previously stated, there has been a general assumption that relaxation and a passive state of mind are favorable to psi. But this assumption was not supported by the (0) and (+1) scores, and it must be noted that even in the case of the significant scores for (−1) in the S_2 (relaxation) condition the deviation was negative. In this case, however, it is necessary to take into consideration that a mental set consciously introduced may not be identical with a mental state which occurs spontaneously.

On the relation between the test score and the change of skin resistance, an interesting phenomenon was observed which is similar to a fact reported by Freeman in his experiment on reaction-time (4): the hits on the direct target increased proportionately to the increase of resistance change, reaching a maximum point at a change of 15 k Ω, and above that value the scores went down. But there were no such definite relationships suggested in (+1) and (−1) scores. For convenience the ESP run scores were divided into two groups: those associated with a change of resistance level of + 10 k Ω and below (called the G_1 condition) and those associated with a change of resistance level of + 11 k Ω and above (called G_2).

Although no significant differences in ESP scores were found by the application of this method to the whole data, some significant differences were observed when it was combined with the above S_1, S_2 analysis. As shown in Table 3, the maximum ESP scores on direct, forward, and backward targets respectively are in S_2G_2, S_2G_1, S_1G_2, and the minimum in S_2G_1, S_1G_2, and S_2G_2. Within

these differences between maximum and minimum scores, those for (0) and (−1) are both significant.

Table 3

ESP Scores Divided According to Mental Set (S_1 and S_2) and the Change of Skin Resistance (G_1 and G_2)

		G_1			G_2		
		Trials	Hits	Dev.	Trials	Hits	Dev.
(0)	S_1	825	158	−7	1025	184	−21
	S_2	1025	167	−38*†	825	178	+13†
	Total	1850	325	−45	1850	362	− 8
(+1)	S_1	792	153	− 5.4	984	181	−15.8
	S_2	984	201	+ 4.2	792	156	− 2.4
	Total	1776	354	− 1.2	1776	337	−18.2
(−1)	S_1	792	160	+ 1.6	984	213	+16.2 ‡
	S_2	984	180	−16.8	792	121	−34.7‡ ‡
	Total	1776	340	−15.2	1776	334	−21.2

*CR = 2.97; P = .003.
†The CR_d between S_2G_1 and S_2G_2 = 2.82; P = .005.
‡CR = 3.32; P = .0009.
‡CR_d = 3.34; P = .0008.

Discussion

As mentioned above, two significant deviations were found in the analysis of the data in relation to the mental and physiological conditions of the subject. In the case of the (0) scores this was observed in mental set S_2 and in skin resistance change G_1, and in the case of the (−1) scores, in S_2G_2. Since this subject showed a significant negative psi scoring level on the (0) score when the (0) scores in this experiment were combined with those in the previous one, it appears that the main factor which caused the negative tendency on the (0) target in this subject was S_2G_1. Why this condition led to a negative deviation on the direct target is not known at present; nor is it clear why the maximum effect on (−1) should be S_2G_2.

These relations between the mental and physiological states and ESP scores may not be constant. It may be suggested, however, that various factors affecting psi processes do not work equally upon every psi function, but each factor works its own effect upon a particular psi function.

This experiment, of course, has no more than preliminary significance, or suggestive value. The facts must be tested further by

more rigidly controlled experiments. This experiment was not sufficiently controlled in its conditions, because the card order did not depend upon random number tables and the signal for each trial was announced by the experimenter himself. As Conklin pointed out, in the measurement of the skin resistance the temperature and the adaptation must be taken into consideration. In my case, it seems that the temperature factor had more influence upon skin resistance than did adaptation. In future experiments, it would be necessary to keep the temperature of the room constant as a further improvement in the procedure.

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A REPORT ON A SENTENCE COMPLETION FORM OF SHEEP-GOAT ATTITUDE SCALE

By R. L. Van de Castle and R. R. White[1]

ABSTRACT: This short exploratory research introduces several new experimental features. In the first place it resulted from the collaboration of two staff workers of the Parapsychology Laboratory and a high school student. Together they planned an ESP test program to be carried out by the student on two groups of subjects composed (except for two adult members) of high school students. It was also a relatively novel development that, although the tests were conducted in a group situation, each subject was presented with an individual target series (sealed in an opaque envelope). The testing started off with one session of four clairvoyance runs. It was followed by a talk by the experimenter and then another session of four runs. The experiment adopted Schmeidler's plan of registering the subjects' attitudes toward ESP in advance of the testing, but the authors went further and developed an attitude scale consisting of nine incomplete sentences to use as a basis for determining more exactly what the subject's attitude was. The ratings were made by the authors in ignorance of the scoring rate on the ESP tests. The 18 sheep (believers in ESP) gave an average score that was above expectation and the 10 goats (unbelievers) gave an average that was below. The 11 subjects rated as having conflict over the existence of ESP averaged at a level between the other two groups. None of the three groups gave significant results but the sheep and goats followed precedent in the general level of scoring. When the sheep and goats were contrasted with regard to the number of sheep scoring above expectation and the number of goats scoring below, the contrast was statistically significant. The research represents an advance toward a new technique of conducting supervised tests in informal laboratory situations.

This is Miss White's first contribution to the Journal of Parapsychology. She is now a Ralph Drake Perry Research Fellow in Parapsychology at Duke University. It is Mr. Van de Castle's second contribution. He has recently transferred from a research assistantship at the Parapsychology Laboratory to a graduate assistantship in psychology at the University of North Carolina. —Ed.

Introduction

A considerable amount of research has been directed toward the study of the relationship between the subject's ESP scoring levels and his attitude of acceptance or rejection of ESP. The most active investigator in this area has been Dr. Gertrude Schmeidler of City

[1] The authors would like to express their gratitude to the Parapsychology Foundation and Mrs. Eileen Garrett, its president, whose financial assistance has made this research possible. We also want to express our considerable indebtedness to Mr. Robert A. Petrof of Beloit, Wisconsin, who solicited the subjects for this test and ably carried out all of the work connected with the actual experimentation.

College in New York. Her published work covers more than 10,000 runs of 25 trials each, obtained from over 1150 subjects who had been classified either as sheep (those who accepted the possibility of ESP) or goats (those who rejected it). She found that, to a significant extent, the sheep, as a group, scored above chance, while the goats, on the whole, scored significantly below chance. Schmeidler's usual criterion for separating subjects into these two attitudinal classifications was based upon a yes or no response to a question asking subjects whether they could accept the theoretical possibility of the occurrence of ESP under the conditions of the experiment. Subjects who reported that they were undecided were classified as belonging in the sheep category.

Attempts to repeat Schmeidler's findings were carried out by Bevan (1), Eilbert (3), Caspar (2), and Kahn (4). All of them found that the sheep scored higher than the goats. In Bevan's case, however, the indecisives scored higher than the sheep, while with Caspar, the indecisives were lower than the goats.

Bevan and Caspar separated their subjects on the basis of a three-point scale used to rate the degree of acceptance or rejection of ESP. Kahn and Eilbert used five-point scales. Schmeidler, as stated above, usually relied upon a single response from the subject, by which he indicated whether he accepted or rejected ESP. In a recent study (5), however, she depended to some extent upon the responses to a series of incomplete sentences, though these were primarily used in rating the subject's degree of annoyance with the ESP test. It seemed to us possible that a more effective attitude discrimination might be made on the basis of a series of questions related to several aspects of ESP rather than by means of a single question. Unless a subject was absolutely convinced that ESP either did or did not exist, the one-answer response would not adequately reflect his attitude. One might also suppose that a question which allowed the subject to express his opinion in words of his own choosing would elicit a more accurate opinion than one which required the yes-no type of answer.

The different results obtained by various experimenters may, we considered, have been due to differences in the methods used to obtain the results. At any rate, it seemed that further exploration was needed to try to develop better methods of separating high- and

low-scoring subjects on ESP tests on the basis of an attitudinal classification. To this end we prepared a mimeographed questionnaire to be given to each subject. The instructions which appeared at the top of the page were as follows:

> Below you will find a series of incomplete sentences which you are to complete by writing down the *first* words that come to your mind after reading the phrase. Don't be concerned with grammatical correctness. For the purposes of this survey that is not important. What is wanted is your immediate response after reading each item. Put down the first word or words that come to your mind.

The following nine incomplete sentences were then presented immediately below the instructions:[2]

1. I believe that hunches often.....
2. Stories we hear about telepathy.....
3. Clairvoyance might sometimes.....
4. The scientific evidence for parapsychology.....
5. People who are skeptical of ESP are.....
6. The idea of telepathy.....
7. There seems little doubt that ESP.....
8. A careful study of parapsychology would show.....
9. It is impossible for ESP.....

The Present Experiment

The first opportunity to test the attitude scale came in the spring of 1955 in connection with a request from Robert Petrof of Beloit, Wisconsin. Mr. Petrof, a senior at Beloit High School, expressed a desire to carry out some experimentation in collaboration with the Parapsychology Laboratory. The authors had planned to give the test themselves, but welcomed the chance to put it in the hands of another person as the actual experimenter. This adds something to the objectivity of the procedure. Such a first attempt is essentially exploratory and its chief merit is the hope that the results may justify further usage. There is need for a sensitive separating device for use in testing small groups of subjects and it may be worth while to see how far this one works.

The experimental design was furnished by staff members of the Parapsychology Laboratory. They also did the rating of the sub-

[2] Experience in rating this scale showed that some of the questions allowed answers to be given which were too ambiguous. A revised scale has been made for use in future experiments. Copies may be obtained by writing to the Parapsychology Laboratory.

jects' attitude surveys and carried out the statistical analyses of the results.

Mr. Petrof was the experimenter and he used two groups of high school students as subjects. One group consisted of 21 high school students, members of the Science Club, who were tested in a classroom after regular school hours. The other group was composed of 16 high school members of the National Honor Society and the parents of one of the members.

The testing conditions for the subjects of both groups were similar in that the testing was always done in a group situation and both groups consisted of approximately the same number of high school students of above average intelligence. They differed in the proportions of boys and girls in each group: there were 28% boys in the National Honor Society group and 71% boys in the Science Club group. The Honor Society group was tested during the evening in the home of one of the students and the conditions were therefore more informal with this group than they were in the testing of the Science Club. There was also a difference in testing procedure which will be described below.

The targets for this experiment were ESP symbols written on a standard ESP record sheet. Each target sheet contained eight columns of symbols which were individually prepared by a member of the Parapsychology Laboratory, using a table of random numbers. There were four columns of symbols under the run headings 1-4 on the sheet and four columns of symbols under run headings 7-10, which left a space of two empty run columns between the two groups of symbols. The target sheet was numbered and placed inside an opaque manila envelope. There were two numbered record sheets stapled to the outside of the envelope with a carbon between them so that the experimenter would have a copy of the data to keep in his possession. Red lines were drawn on the outer record sheet to indicate the ending of the fourth column and the beginning of the seventh.

To each of the two groups of subjects, Mr. Petrof gave a brief introductory talk on parapsychology which included explanations of telepathy, clairvoyance, and similar terms. The attitude scale was then administered and collected by the experimenter. Following this, the experimenter distributed the prepared envelopes to each

individual and the subjects each completed four runs at their own speed. They were then given an intermission during which a second talk was made by Petrof. This time he stressed the fact that negative scoring, as usually produced by goats, actually demonstrates ESP as meaningfully as the positive scoring of the sheep. It was pointed out that the way to disprove ESP was not to score low but rather to obtain chance scores.

When this talk was finished, the subjects completed the remaining four runs, tore off the original record sheet, and passed it to the experimenter for official scoring later. Each subject then exchanged his envelope for that of another student. This other student took out the target sheet, wrote down the target order on the carbon record sheet, and passed the target sheet in to the experimenter. The number of hits were totalled on the carbon and returned to the original subject so that he could see how he had done on the test.

The procedure outlined above was followed for the National Honor Society (NHS) group, but for the Science group the administration of the test was broken up into two sessions. This latter group completed four runs during the first session. A week later the second talk was given and the remaining four runs were carried out.

The experimenter forwarded the attitude scales to the Parapsychology Laboratory where two staff members jointly rated the subjects according to their belief classification. This was accomplished by assigning a weight of 1 to what was considered a negative attitude toward ESP, a 2 to a neutral position, and a 3 to a positive orientation toward ESP. This was done for each individual sentence. For example, in answer to the question, "A careful study of parapsychology would show . . . . ," a weight of 1 was given for the response "be stupid," a weight of 2 for the answer "something," and a 3 for "that it exists."

These weights were then totalled and subjects scoring above the arbitrarily chosen point of 19 were labelled sheep while those scoring below 17 were called goats. These points were determined as being one point above and below a theoretical point of 18 (9 items times a rating of 2 each). This arbitrarily fixed basis was employed, since the scale had never been used before and there

was no way of telling, in this first study, which points would yield the best separation. At this time, they had no knowledge of the ESP scores. The records of those subjects who scored between these two cut-off points were re-examined to see if their over-all responses favored a sheep or goat attitude. A few of these subjects whose responses consistently favored a particular classification were added to the appropriate category but the remaining subjects gave contradictory or ambivalent responses and therefore were labelled "conflict" subjects. Such a subject might, for example, say "There seems little doubt that ESP *is true.*" in one sentence and then answer "It is impossible for ESP *to be possible.*" in the next.

At the time when the subjects were rated according to their attitudinal classification, the judges had no knowledge of the ESP scores. After the ratings were made, a duplicate copy was sent to Petrof. He checked the ESP scores and divided them according to the sheep-goat-conflict classification which had been sent him. He then mailed the results of this separation, together with the original ESP record sheets, to the Laboratory for independent checking. There was a lapse of a week between the time the rating list was sent from the Laboratory and the time the ESP record sheets were received. The authors recognize that in future tests better control of this aspect of the experiment could be provided by having the rating sheet and the ESP scores cross in the mail, with each experimenter retaining a carbon copy of the material he sent so that he could have a complete set of data for checking.

From the total of 312 runs of 25 trials each, there were 1543 hits, which gave a negative deviation of -17, or an average run score of 4.95. The 168 runs of the Science Club gave a deviation of -15 and an average run score of 4.91. The 144 runs carried out by the National Honor Society gave a deviation of -2 and an average run score of 4.99. These results are, of course, not significant.

The sheep averaged above mean chance expectation in both groups (5.09), while the goats averaged below (4.77). However, the difference in average scoring level ($CR = 1.15$) was not significant. The data are summarized in Table 1.

In order to evaluate the consistency of the scoring obtained by the sheep and the goats, the results were arranged in a 2×2 con-

Table 1

AVERAGE RUN SCORES ACCORDING TO ATTITUDINAL CLASSIFICATIONS

Attitude Classification	SCIENCE GROUP			NHS GROUP			COMBINED		
	Number of Runs	Number of Subjects	Average Score	Number of Runs	Number of Subjects	Average Score	Number of Runs	Number of Subjects	Average Score
Sheep	96	12	5.13	48	6	5.02	144	18	5.09
Conflict	32	4	4.63	56	7	5.00	88	11	4.86
Goats	40	5	4.63	40	5	4.93	80	10	4.77
Total	168	21	4.91	144	18	4.99	312	39	4.95

tingency table. This gave the number of subjects whose total score was above chance, and the number of subjects whose scores were at or below chance expectation, following the basis of grouping that has been established by practice. Since small frequencies were involved in the marginal totals, the results were evaluated by the exact method of obtaining the probability. Of the 18 sheep, 11 scored above chance while out of the 10 goats, 9 scored at or below chance. The probability of such a distribution occurring by chance alone is 1 in 91. See Table 2.

Table 2

SCORING LEVELS OF THE SHEEP AND GOAT SUBJECTS

	+	0 or −	
Sheep	11	7	18
Goats	1	9	10
Total	12	16	28

$$P = \frac{18!\ 10!\ 12!\ 16!}{28!\ 11!\ 7!\ 1!\ 9!} + \frac{18!\ 10!\ 12!\ 16!}{28!\ 12!\ 6!\ 0!\ 10!} = .011$$

DISCUSSION

In this experiment, as in Schmeidler's work, the sheep were found to score above chance while the goats scored below. When the two groups are contrasted on the basis of the number of subjects who scored above mean chance expectation and the number who scored exactly at or below this level (see Table 2), the ratio of

above-chance subjects among the sheep is significantly higher than among the goats.

This analysis is the appropriate one for testing whether there is any difference in ESP performance associated with attitudes of belief or disbelief. The group averages presented in Table 1 do not bear directly on this point; they are offered merely to provide a more complete picture of how the results were distributed insofar as the sheep, goats, and conflict subjects were concerned.

The results were also analyzed according to the number of hits obtained for the first four runs as compared with the second set of four runs which followed the talk about the interpretation of negative scores. From the results, shown in Table 3, it is apparent that the talk did not produce any significant change in the scoring level.

Table 3

Comparison of Scoring Level for Runs Preceding and Following the Second Talk

Attitude Classification	Science Club				NHS Group			
	Number of Subjects	Hits in First 4 Runs	Hits in Second 4 Runs	Mean Chance Expectation	Number of Subjects	Hits in First 4 Runs	Hits in Second 4 Runs	Mean Chance Expectation
Sheep	12	247	245	240	6	121	120	120
Conflict	4	74	74	80	7	139	141	140
Goats	5	87	98	100	5	104	93	100

This study was only intended to be exploratory, and therefore the results have only suggestive value. It is encouraging, however, to find that the results, in spite of the limited scope of the experiment, are consistent with the findings of Schmeidler and other investigators on the relation between attitudes and ESP performance. This study may point the way toward collaboration with interested but inexperienced experimenters which might show in future research whether the sheep-goat effect is one that can be consistently and reliably demonstrated.

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EDITORIAL

SPONTANEOUS PSI RECONSIDERED

No apology is needed in parapsychology or in any field of science to preface the investigation of spontaneous natural phenomena. In the case of parapsychology there would not have been a field of organized psi research had there not first been the reports of "psychic" experiences. In this, as in many branches of inquiry, an anecdotal stage came first. The spontaneous occurrences raised questions which led to suggested hypotheses (or beliefs), and these in turn brought about efforts to test their soundness experimentally. The experiments were then repeatedly confirmed.

But when the experimental stage is reached, what then becomes of the spontaneous material? Ideally it should still continue to raise questions as long as any mystery remains concerning this type of occurrence. Eventually, however, scientific knowledge should advance to the point at which there are adequate explanations to account for all the spontaneous manifestations of the field.

There are departures from this ideal course of development, and these need to be watched lest they lead to impasses or fruitless wanderings. Most common perhaps is the tendency to drag out the anecdotal stage so that it becomes an end in itself. This is most likely to occur when a group is devoted to a certain interpretation or philosophy built on such material. The history of certain religions, philosophies, and associated practices provides a wealth of instances exemplifying this tendency to focus on the exceptional mystifying experience, to conserve it as genuine truth, and to withhold it from the more developed methods of inquiry. A great amount of spontaneous material has thus been woven into conservative systems of belief and practice in such a way that it is very hard to evaluate or recognize it for what it is.

An equally extreme reaction in the opposite direction is some-

[1] This editorial is based on a paper read at the conference on spontaneous cases held by the Society for Psychical Research at Cambridge University, July 11-17, 1955. I wish to acknowledge with gratitude a travel grant from the Parapsychology Foundation of New York which made my attendance at the conference possible.—J.B.R.

times observable on the part of experimental enthusiasts who distrust all evidence not obtained under controlled conditions. They become impatient with the case report, the personal experience, and the anecdote. At a certain stage in the history of most branches of natural science the expression "anecdotal material" becomes a term of disparagement. This period is usually a stage marked by the effort to secure scientific recognition and status for the field. Emphasis on the "controls," "rigorous precautions," and "repeatability" of experiments is likely to dominate such a period. At that stage spontaneous cases are taboo.

These opposing extremes in attitude toward case material can be found at present to a noticeable extent in and around parapsychology. Clarification is therefore needed as to the distinction that is implied in the term "spontaneous occurrence," and what purposes may be served by maintaining it. It happens that in connection with the studies L. E. Rhine has been making at the Duke Parapsychology Laboratory it has been necessary to re-examine the place of spontaneous psi experiences in the research field of parapsychology at large. The following outline of this re-appraisal may serve as a basis for critical review by others.

The first step in clarification has to do with definitions. Obviously one's attitude toward spontaneous psi experiences will depend upon what the term "spontaneous" means to him. To most students of parapsychology the spontaneous case is set off in contrast to the experimental results; and, of course, that distinction has in the past been a useful one. It is, however, a loose and questionable classification for the more refined needs of the present. For example, even with the most elaborately controlled psi experiments, the operation of psi is still in one sense of the word *largely* spontaneous. That is the reason why statistical method has to be used to distinguish the uncertain functioning of psi from the random events of chance. The elaborate experimental procedures in use have been designed to register fugitive psi effects when and as they occur, and to distinguish them from other operations which may obscure them. But even with the best efforts of the best subjects working under the best known experimental guidance, there is still today a large and unfortunately predominant element of spontaneity in the individual's performance.

There is at least one important difference, however, in the experimental and spontaneous approaches to psi. The experimental point of view assumes that psi effects will, at least with some measure of probability, be repeated; that is, that even though its recurrence cannot be brought about on demand, the phenomenon is a reproducible one. And yet the *mere property of recurrence* is not a thoroughly clear-cut difference between the two kinds of psi phenomena. For example, quite apart from experiments, a number of non-experimental areas of *practice* exist in which the recurrence of some such capacity as psi has been assumed—as in automatisms such as dowsing (with the use of "divining rods," "exploratory pendulums," "dual locators," and the like). Among such practical efforts belong also the claims of unconventional healers which appear to depend upon the operation of a function equivalent to psi. The practices of mediumship and fortune telling are in a similar class. All of these non-experimental arts and practices, whatever their validity, assume that there will be a recurrence of whatever psi-like function they claim. Hence, it is not only in experimental psi that it is assumed the phenomena will be to some extent reproducible.

But whether or not there be recurrence will not serve as a basis for distinguishing even among the more spontaneous types of occurrences. For the tendency of the phenomenon to repeat itself, however irregularly, characterizes quite a diversity of manifestations commonly considered to be of spontaneous psychic character. As reported, all the broad categories of hauntings, for example, are repeated. (A house would not be considered haunted on the strength of a single apparition!) The same is true of poltergeists. In fact, as they are described, recurrence is a featured aspect of these occurrences. There are even instances among hauntings and poltergeists in which the phenomena are reported to recur in response to request or solicitation. This introduces a new distinguishing feature. Finally, there are numerous examples of repetition among common types of even the more personal psi experiences; for example, a premonitory experience in connection with the death of a family member may be followed by a like experience in connection with succeeding family deaths.

It is plain, then, that recurrence cuts straight across the line of distinction between what has been termed the spontaneous and the

nonspontaneous. Likewise, spontaneity itself spreads out over any distinctions which for convenience have been used in the field, varying with these distinctions only in a matter of degree.

Is there, then, any characteristic of experimental psi that *does* serve to differentiate it, not only from these recurrent spontaneous phenomena, but also from the field of psi practices already mentioned? One such characteristic would be that in experimental psi a known living subject consciously tries to produce a certain designated effect. The word "consciously" is necessary because in some of the more spontaneous cases of recurrent psi (for example, poltergeists), there is a suggestion of unconscious design (even by some unsuspected individual in the locality).

It would, likewise, seem better to say, "a known *living* subject" because certain recurrent spontaneous phenomena are interpreted as centering in discarnate personalities. These are recognizably arbitrary distinctions. Experiments might be conducted with hypothetically nonliving (i.e., spirit) personalities, if such exist. But it could have been said above that it would be equally possible too to conceive of doing experiments with unconscious subjects. The division between experimental and spontaneous is a temporary surface one, serving only the needs of the day. It is enough to say that in experimental psi there is conscious effort by a living subject to produce, under the conditions designed for the experiment, results attributable to a potentially repeatable phenomenon.

What, then, distinguishes experimental psi from the area involving psi practices? This latter area clearly needs distinction, if only because today much more human effort is devoted to it than to experimental psi itself. It is not difficult, however, to distinguish the comparatively uncontrolled practices that are assumed to involve psi from the experimental demonstrations of that capacity. The essential difference lies neither in the performance of the subject nor in the character of the manifestation produced. Both in practice and in experiment a conscious living subject (or practitioner) is involved, and in both is involved an assumption that the phenomenon can be induced to recur. Rather, the difference lies in the design of the conditions under which the ability is to function; in the experiment the design is intended to evoke effects that could be attributed *only*

to psi. In a practice, on the other hand, the need is only for conditions that *allow* a possibility of psi.

From this attempt to classify types of psi effects according to their degree of spontaneity, four divisions emerge. While their outlines are comparatively clear-cut they are still at best only broad approximations. They will serve, however, to show the general differentiating features of psi phenomena when graded with respect to the extent of control involved. These more or less overlapping divisions are summarized below and are also represented more diagrammatically below.

Class I, *Simple Spontaneous Experiences,* is composed of the more completely spontaneous psi occurrences. These are not consciously solicited and are nonrecurrent so far as the individual is concerned. The fact that they may be no different than some which may recur in other individuals is beside the point. So, too, is the fact that they have been in a way recurrent throughout the history of mankind.

Class II, *Recurrent Spontaneous Effects,* also would consist of occurrences not consciously sought by the percipient or observer, but which do, however, recur, even though the repetition be highly unpredictable. Here belong all the many forms of unsolicited but repeated warnings, hauntings, poltergeists, and other such manifestations

Class III, *Psi Practices,* combines all the many arts and performances of men and animals that appear to depend on psi. It would include, of course, only natural, nonexperimental performances. The effects are both recurrent and solicited but, of course, the boundaries are flexible. It would be a long list indeed that would contain all the practices of mankind throughout history which have claimed or have appeared to rely on some such ability as psi. All the arts of mediumship, divination and the mantic practices of healing or of causing injury would belong here. Practical devices of object- or direction-finding by means of psi ability likewise qualify. Even the more remarkable distance orientation feats of animals (e.g., homing) would belong if they should prove to depend in any way or degree upon psi. They would be practical performances in a

general class with the dowser, whose claim would place him in this section.

In Class IV, *Experimental Psi,* belong the entire range of experimental psi manifestations. These effects are also, of course, both recurrent and solicited and in addition are produced under controlled conditions. Some of the lines developed by experiment were based on or suggested by Class III, just as others have been derived from more purely spontaneous psi occurrences. While the emphasis in Class IV is on external control (that is, on the exclusion of non-psi factors), it includes also whatever range of *internal* (psychological) control over psi increased understanding may make possible. As control improves, the range of spontaneity remaining will be still further reduced.

GRADE OF SPONTANEITY VS. CONTROL IN PSI EFFECTS

I	II	III	IV
SIMPLE SPONTANEOUS EXPERIENCES	RECURRENT SPONTANEOUS EFFECTS	PSI PRACTICES	EXPERIMENTAL PSI EFFECTS

So-called Spontaneous — Solicited

Recurrent

There should in time be added a Class V division, to include those examples in which the conditions for solicited recurrence have been so well controlled as to produce dependable results within a limited test or practical situation. No one can say whether the point at which there would be no spontaneity left will ever be reached even though that must be the ultimate goal of research.

* * * * *

If the above analysis serves its purpose of clarification, certain general impressions should be fairly apparent. The investigator wanting the most reliable answer to his questions will logically operate as far as possible toward the experimental end of this four-section distribution. Obviously, the more he can cause the phenomenon to recur under voluntary effort, the better able he should expect to be to control the experiment and interpret results. Of course, no line can be drawn at which it could be said that types on the right end of the distribution would be evidential and those on the left, nonevidential. At the same time, the difficulty of securing

reliable proof increases toward the purely spontaneous end of the scale.

If, on the other hand, the explorer is in search of new problems or new hypotheses, the case is quite different. For this purpose the more spontaneous end of this range of phenomena is likely to be much the richer in potential and, because of its unexplored effects, can promise fresh ideas and new orientations. A wider range of inexplicable happenings are to be expected in the less frequently reproduced and the more unsought-for effects that occur without efforts at control introduced by practical or experimental design. This would be true in any field in which the spontaneous natural phenomena were still largely unexplained.

It would not follow, however, that Class I would be the most fruitful place for every worker to decide to begin his research. Many other considerations would have to enter into such a decision. New problems challenge investigation all along the line from the most to the least spontaneous class of phenomena. For example, almost every experiment raises new questions and all the psi practices likewise present almost ready-made issues for investigation. Indeed, one should turn to the more spontaneous type of source material, not to look for answers to questions already raised and formulated as hypotheses, but rather when in search of renewed perspective, a more original approach, and the stimulus of new questions. This kind of reorientation can, within limits, be very important to the progress of research.

When the inquirer does gain from a study of the more spontaneous order of psi experiences some new insight which he can formulate as a hypothesis, he should remind himself that it is at best only a suggestion, a potentially good idea that must still be validated (or invalidated, as the case may be) by proper experimental test. This final screening is, of course, the safeguard, the entrenched position to which this exploratory worker must fall back with his new and still tentative suggestions. But the securing of a new working concept is an essential creative part of the scientific process.

The collecting of case material requires, for one thing, that the burden and expense and effort of collection be reduced to a reasonable minimum; but more important, that the scope and range be comparatively unrestricted. By recognizing the limited evidential

value of individual case reports and by shifting to the experimental stage the emphasis on verification, the necessity for the retarding and costly processes of attempting to authenticate can be largely obviated. A tentative brief judgment of the reliability of case material can be made as a matter of course as it comes to the collector's hand, and this would be sufficient. A case analyst can soon acquire a quick preliminary gauge of the tone of responsibility, balance, sincerity, and the like, and screen from the collection at the start a variable amount of meaningless material likely to be swept up in any large scale collection. With such free methods of appraisal and classification probably between one hundred to five hundred times as many case reports can be handled effectively as was possible by methods devised for the time when cases were studied primarily for their evidential value.

The value of a collection will depend, not only on its magnitude, but even more upon its breadth. Therefore, there should be no rigid or narrow standards of collection that would bar out whole types of experiences that might reveal clues the experiments themselves have not yielded. It would, for example, be easy to overlook cases of uncertain significance at the time of occurrence since they would not be properly recorded. Accordingly, no stiff demands should be made regarding such a requirement. Cases from nonliterate cultures too would never qualify unless the standards of acceptance were adapted to the nature of the situation. In fact, the only sure way to acquire an adequate collection is to welcome all case material for whatever it may suggest. If the search for psi characteristics is to be effective, there must be no arbitrary rejection at the admission gate of anything related. That would defeat the purpose of the inquiry at the start.

* * * * *

It seems unlikely now that parapsychologists will ever again lose sight of the rich source of stimulation and guidance inherent in case reports until they (with the help of others) have fully cleared up the mystery of spontaneous psi experiences. Research workers of the future will doubtless wish to know all they can of the more natural, as well as the more controlled, methods by which psi effects can be produced. It is safe to say also that as a suitable storehouse or library of more spontaneous types of experiences grows

into full usefulness, investigators will draw increasingly upon it for their strategy in designing new programs. It is, in essence, a way of bringing Nature herself to a place at the planning table.

There are, however, other advantages of large scale case studies hardly less important than that of furnishing new ideas. One of these that may be worth a great deal, if taken with proper restraint, is the possibility of checking the experimental findings against the background of comparison provided independently by the cases. In the event that an experimental finding represents a highly challenging discovery (as is usually the case in parapsychology), an analytical study of large masses of the data of human experience embodied in case reports can, if it confirms the experimental results, offer a great deal of rational reassurance. Such confirmation would not, of course, have the same kind of value as another experimental verification. It would provide, rather, a certain reassurance to find that a conclusion reached by the purely experimental procedures was evident too in Nature's own portrayal. For example, when L. E. Rhine made a study comparing contemporaneous clairvoyance with precognitive experiences and found no discoverable subjective difference between the two categories, it added to the understanding of the case for precognition as a whole. While the spontaneous material would not by itself have been at all adequate to establish a case for precognition, once that had been experimentally demonstrated, this secondary support contributed more than a little to the rational appraisal of that most revolutionary of all the findings of psi research.

This new respect for the potential of case material grew mainly out of the security felt since the adequately safeguarded methods of verification have been introduced. Parapsychologists would not otherwise be able to use case material with freedom and confidence. As it is, there is reasonable certainty that nothing arising from a study of nonexperimental occurrences will escape the screening methods of verification in use today or reach the conclusive stage without due testing.

Likewise, all those who have been afraid that experimenters may be unduly narrow in their scope of study should be relieved to know that no type of psi experience, in whichever of the four classes it belongs, will long be neglected in the future. The whole broad field

is coming under survey. As to how productive each study of cases, or practices, or demonstrations may prove to be, and which class of psi activities will turn out to be of greatest suggestive value, it would be impossible to say. The main point is that nothing potentially valuable to the whole program of parapsychology is likely to get lost under the new program as now conceived—a program for the eventual collection and study of the whole range of psi manifestations reported. As this re-examination of psi-in-nature develops and broadens, it should ideally extend to whatever unexplored area remains in the study of the nonphysical functions of mental life.

Finally, another value remains that must not be ignored. At this stage of the development of the field of parapsychology, the closer the acquaintance of the experimenter himself with psi in its more spontaneous forms, the better he can gain a vivid and realistic appreciation of the capacities he is attempting to investigate. Just as the interest of the general public in the laboratory investigations has been partly sustained by familiarity with spontaneous psi happenings, so the Laboratory worker can add to his own enthusiasm as well as to his information from a first-hand knowledge of those far-ranging human experiences so evidently related to his experiments.

J. B. R.

LETTERS AND NOTES

PARAPSYCHOLOGY AND THE CONCEPT OF "RANDOMNESS"

J. Fraser Nicol's citations in a recent article in the *Journal of the Society for Psychical Research*[1] implying that all statistical definitions of "randomness" are necessarily circular, may suggest, especially to non-mathematical readers, that the concept of "randomness" in applied probability theory is "vague" or imprecise in a sense urged by G. Spencer Brown.[2] To entertain the suggestion, however, would be to misconceive the contemporary situation in mathematical statistics. I am sure that the misconception is not intended by Fraser Nicol. Modern mathematical theory tends to be largely axiomatic and postulational. With "indefinables" or dummy-terms (for example, "points") as relata, it builds up relational structures of various kinds and remains unconcerned about their empirical applications. Bertrand Russell's epigram, which Nicol has quoted, that mathematics is the subject in which we never know what we are talking about nor whether what we are saying is true, enforces the important lesson about the "indefinable" and non-empirical character of mathematical relata.

The popular impression that the mathematical concept of probability necessarily involves an odd mixture of empirical and non-empirical elements is misleading. Since the Lebesgue theories of measure and integration were proposed and freed from certain geometrical associations by Fréchet, Kolmogorov has shown how the analogies between the measure of a set and the probability of an event, the integral of a function and a random variable, can be convincingly developed on purely axiomatic lines. Various properties of independent random variables can be shown to be completely analogous to the corresponding properties of orthogonal functions. Without entering into all the niceties of the theories expounded by writers like Doob, Feller, Neyman, and Cramer, it may be said that probability can be regarded as the property of certain point-sets,

[1] June, 1955, pp. 73-74.
[2] *Nature*, Vol. 172, No. 4369, pp. 154-56.

Borel sets to be more precise, with specified properties. Consistently with the axioms, it can be shown that probability, for any set of points, is non-negative and additive and, for the set of points representing the whole "sample space," is unity.[3] From the axioms can be deduced various important theorems, one of which, namely the central limit theorem, states that almost regardless of the true distributions of successive random variables, x_1, x_2, ..., the sum has a distribution which tends to a distribution of known form, depending on the mean and variance of the distribution of the x_1's.

The pure mathematician may, of course, have nothing to say about a particular phenomenal interpretation of probability as "relative frequency." Nevertheless, there is something to be said from the standpoint of applied mathematics. Spencer Brown's recent correspondence in the *Journal of Parapsychology*[4] shows how seriously he has underestimated the significance of the Greenwood "control" series. Sampling from a binomial population may be regarded as a special case of the central limit theorem; it may then be shown that the probability that the relative frequency deviates from a particular value by more than a certain amount tends to zero with increasing sample size. In the Greenwood check series consisting of 20,000 runs or 500,000 card-call pairings, the observed values of the average, the standard deviation, skewness, kurtosis, the third and the fourth moments, were in substantial agreement with the theoretical values of the binomial distribution. Indeed, the binomial hypothesis emerged more convincingly from the check series than from Weldon's classical dice data. I have elsewhere[5] remarked that more general hypotheses about distribution, in the "card-calling" situation, yield far less information than the binomial hypothesis.

Experiments eliciting apparently nonrandom results from random numbers are interesting and should serve, as Nicol has observed, to put us on our guard against possibly incautious uses of random numbers. But they cannot shake our confidence in statistics used in the Soal-Goldney type of experiments. The "cross-checking" of these experiments tells overwhelmingly against Spencer Brown's theory. The issue is not one about *theories* of "randomness"; lingering doubts about the precision of the current concepts

[3] See my letter in the *J. Soc. psych. Res.*, July-October, 1954, pp. 357-58.
[4] March, 1955, pp. 58-59.
[5] *Proc. Twenty-eighth Indian Philosophical Congress*, 1953, pp. 133-37.

of mathematical probability can be dissipated. Most, if not all, of the theories are equivalent in their *practical* applications. It is the inadequacy of applied statistics in the Soal-Goldney type of research that Spencer Brown has to establish. That he has lamentably failed to do this should be obvious.

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PRECOGNITION AND INTERVENTION

I wish to present here some remarks suggested by the article "Precognition and Intervention" by Dr. L. E. Rhine.[1] These comments are based upon experimental observations concerning the problem of precognition and free will.

I wish first to emphasize my conviction that correctly precognized events are bound to occur in spite of any effort at intervention. I think this is inherent in the meaning of precognition, as Dr. Rhine correctly recognizes. It seems to me, however, that the problem of the relation between precognition and an assumed freedom of the will must not be formulated in terms of whether or not intervention in precognized events is possible, but rather in terms of the following two questions: (1) Is every future event precognizable? and (2) Is it not possible to cognize also a mere probability of an event which is not inevitably bound to occur? Maybe this could be the new approach to the problem Dr. Rhine is calling for.

Dr. Rhine gives in her article examples of cases in which the precognized event was fulfilled in spite of all attempts to intervene. The reasons why the intervention was not successful were given as follows: (1) The information obtained was too vague. There was lacking a precise statement about either the person, time, or place, etc. (2) An uncooperative person was involved. (3) The situation was unpreventable (e.g., in natural cataclysms).

Similarly in those cases in which the intervention seemed to be successful, the case for an averted destiny was not clear because of these possibilities: (1) alternatives to precognition were possible;

[1] *Journal of Parapsychology*, March 1955, pp. 1-34.

(2) the precognized impression was vague; (3) the precognized impressions were specific but too limited; (4) no specific mention of the individual had been made; or (5) escape was possible without intervention.

Even the nine best cases, therefore, as Dr. Rhine herself recognizes, were inconclusive. It is true, in the study of precognitive and related experiences we often find the impressions distorted. Thus we may consequently suppose that in those cases found acceptable as intervention cases, the real precognition was confined to that part of the impression which *was* later fulfilled. That part of the impression in which the intervention apparently succeeded may have been no *real* precognition of a factual event, but only a cognition of the *possibility* that this event would happen (e.g., the possibility of collisions in examples 31 and 32 in Dr. Rhine's article). This possibility is included in the framework of the precognitive experience as a whole, however, and becomes (possibly in a symbolic form) part of the vision of a real accident.

I think the problem of precognition and intervention may be more successfully attacked in the second way Dr. Rhine suggests: by studying the properties of precognition and its possible limitations. It must be done, I think, by assuming an absolute determinism in the following sense: that *what is correctly precognized is bound to occur* no matter what efforts to intervene may be made.

Therefore, if an event in the future is reliably cognized, it will be fulfilled in spite of any effort to intervene. There may be cases in which not all the accessory circumstances are foreseen, and therefore the precognized event may find its fulfillment in an unexpected way. In this connection I suspect future research may show that some kinds of events are not likely to be reliably precognized; I mean those in which an intervention is still possible and likely. In addition to this, I think it possible that besides the events in the future which are bound to occur, there also is left a place in the future for events "in preparation" (that is, potential events) in which by the interference of free will a choice between two or more alternatives may be exercised.

It is possible that such potential events, both those which will come to realization and those which will not, are also to a certain extent accessible. But it is necessary to find an experimental cri-

terion to distinguish them from the true precognition of predestined events, those which are bound to occur.

Some years ago I performed several exploratory clairvoyance experiments with hypnotized subjects who sometimes showed very reliable clairvoyant abilities. There were also several cases of precognition which are relevant to our problem. First, I observed that on various occasions events were apparently correctly precognized in whose course I could, at least theoretically, interfere, but in which (whether or not because of my curiosity to find out if the precognition would come true) it was certain beforehand that no one would so interfere. For instance, circumstances were predicted in which a certain person would be lightly injured in an accident. I knew in advance the name of the person, the precise time, place, circumstances of the accident, and the future progress of the injury. I had (at least, so I thought) the possibility of warning the person in question, or even of compelling her to evade the danger. However, in view of the circumstances, I preferred not to do so. In another case, when I tried to obtain precognition of the time at which a watch would stop, I could have tried to avert the fulfillment of the precognition; that is, I could have broken the watch to pieces. I did not do that but waited until the time the watch stopped and thereby showed the correctness of the precognition. To tell the truth, if I had interfered with the predicted happening, and if this effort to prevent the fulfillment of the precognized event had proved to be successful, I would not have been certain that I really had interfered with a precognized and predestined event. The prophetic impression in this single case might have been only a meaningless hallucination. Any intervention would have been uncheckable in these cases and therefore was not attempted.

The following observations illuminate our problem a little more:

(1) One of the hypnotized subjects with whom I experimented believed she was able, by judging the distinctness of her clairvoyant vision, to distinguish uncertain statements from certain ones. This subject informed me that she was able to predict reliably all events in the far future, that is, 50-60 years or later. Of course, these statements could not be checked. With respect to events in the nearer future, I was told she could correctly cognize events which depended upon the intersecting efforts of many individuals. Those

events which depended on the action of a single individual were seen indistinctly and therefore could not be predicted quite reliably.

(2) Another group of experiments was designed to test the precognition of events which could be influenced by a decision of a single person. Unfortunately, the experiments were performed with a subject who was not able to remove satisfactorily all sources of error—whose clairvoyant ability, therefore, was not sufficiently reliable. Although the result of these experiments was negative I wish to describe them because this might give an impulse to others to attempt to repeat them more successfully and perhaps to bring more light on our problem.

I had chosen a series of future sport matches as the means for determining targets in an experiment. After these games were over, an assistant was to indicate the results of each match by a code of easily recognizable movements of her body and limbs. A certain movement was assigned to every possible result. Before the games were played, the clairvoyant subject was to predict the movements the assistant would perform. It is obvious that the experimenter could not know in advance which movements would become the target because it would depend on the way the games came out. The assistant who finally made the movements was not told which of them had been foretold.

The experimental procedure was as follows: For instance, twelve games would be chosen: A contra B, C contra D, E contra F, etc. In the first modification of the experiment the assistant was told to go at a specified time, after the games had been played, to a specified place, and there to indicate the results of the games—let us say, in the following way: lifting the left hand to indicate that A had won; lifting both hands to indicate a tied score; lifting the right hand to indicate that B had won. Next, lifting of the left hand indicated that C had won; lifting both hands, that it was a tie; lifting the right hand, that D had won; and similarly until the results of all the twelve games were indicated. The subject was told to determine by ESP of the future the position and all the movements of the assistant. Thus, if he could manage it correctly, the experimenter could, after decoding the meaning of the movements, learn the results of the games in advance.

After the negative result of this experiment, I tried an improvement by ordering the assistant to maintain the positions for a longer time. For instance, the hand had to be held up without movement at least for one minute; and as a chief improvement, each game was distinguished from others by various outstanding characteristics. This I considered necessary because in other experiments I have seen that when such a complex scene is to be cognized, the subjects are often not able to describe the movements in their right succession. They often make "jumps" in time; for instance, they miss several movements and then unnoticeably return to preceding ones. So in the last modification the assistant was to indicate the results of the games in the following way; for the first game she was to dress in a white blouse and black skirt and while sitting on a chair, she was to lift the left hand to indicate that A had won, the right hand to indicate that B had won, and both hands to show a tie. For the result of the second game, the assistant was to put on, for instance, a black blouse and white trousers and to indicate the results as she stood on a table in another corner of the room. In this case, putting the left hand on her right knee would indicate that C had won; covering her face with both palms would indicate a tie; and sitting down on the table, that D had won. For the third game, the assistant was to be dressed in black trousers and a black blouse and to make the following indications as she lay on the floor: lifting both hands to show that E had won; lifting both legs, that F had won; lifting both hands and legs to show a tie. Similarly for all other games a different dress, different place, and different movements were chosen. The subject had to predict each movement and also all accessory circumstances: dress, position of the assistant, etc.

In spite of these methodological improvements the experiment, repeated several times, never led to a successful result. The clairvoyant subject was never able to predict the movements reliably. Two explanations for the failure can be presented: (1) It is possible that this subject had not sufficient ability to make these predictions. (2) It is also possible that the predicting of these movements was impossible. If this second explanation be correct, two possibilities come to mind. First, this was an event in the future which was still undetermined at the time the foretelling took place, and it may be doubtful whether such events are precognizable. Judging

by my previous experience, however, I think that such events *are* cognizable in some way. Second, the event was entirely dependent upon the free determination of one single person, either the experimenter or the assistant (as to whether or not he would eventually perform the movements). The movements were determined according to the results of the games, and on the basis of these movements, the experimenter would be able to decode the results of these games. The arrangement of these experiments was such as to make possible a kind of signaling of the results from the future to the past.

If in this way the subject had predicted the results of the games correctly, the exercise of free will could have brought the experiment into a paradoxical situation. For example, after the results of the games had been correctly predicted, the experimenter could have ordered the assistant to perform no movements at all, and then the results of the games would have been correctly predicted on the basis of movements which never had been performed.

I wish to mention these suggestions which, in my opinion, speak for the hypothesis that future events which could be modified by free will are not accessible to precognition. These experiments were performed four years ago and I am not able to offer more experimental material just now. I hope I have at least suggested an experimental approach to this problem that may not be too remote when a subject with good precognitive abilities is available. The experimental approach to this problem may be more promising if the targets for cognizing are not simple objects (as for instance ESP cards) which enable statistical treatment of the results, but rather more complex situations from daily life.

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Errata

The following corrections should be made in the Journal of Parapsychology for June, 1955 (Vol. 19, No. 2):

P. 86. Table 1, under the heading P: *0.13* should read *.013* and *0.17* should read *.017*.

P. 94. Table 2, under the heading CR: *4.42* should read *4.59*.

P. 95. Line 1. *square of the distribution of hits for the four quarters is 6.60 (d.f. = 3), which gives a P value of between .05 and .02* should read *square of the distribution of hits and misses for the four quarters is 9.28 (d.f. = 3), which gives a P value of between .03 and .02*.

P. 96. Line 5. *($\chi^2 = 6.2$; d.f. = 4), giving a P value of between .01 and .02* should read *($\chi^2 = 11.79$; d.f. = 4), giving a P value of .02*.

P. 97. Table 3, under the heading CR: *4.10* should read *4.16*.

P. 97. Table 4, under the heading CR: *1.90* should read *1.74*.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD = 2 $\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ^s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

of the spirit. Whatever name psychology may give it, and however we may try to account for what it does, we should not let a naive materialism persuade us to neglect as useless or unreliable this spiritual avenue to reality.

Here is the issue joined between those who see in man no more than a physical mechanism, however marvelous, and those who are convinced that through this spiritual part of him there is to be heard at least a whisper of a reality beyond the world of time and space and matter that so engages us on every hand. Which side each of us will take is governed more by inner conviction than by argument, but the decision is momentous, for whether religion has real meaning or is but wishful thinking depends on what we believe about the spirit of man. If all the goal-seeking, desiring, and aspiring of our spirits are but curious tensions in a protoplasmic mechanism, they are still of interest and importance in many ways; but if, beyond all this, they provide a means of communion with something in the universe that can be apprehended in no other way, then does their significance indeed transcend all other human qualities.

Edmund W. Sinnott, *The Biology of the Spirit**

* Published by The Viking Press.

It seems more probable, however, that radically new ways of looking at nature may have to be opened, ways that are so different from the familiar ones science long has trod that many will refuse to recognize them. What are we to say, for example, to the abundant evidence that telepathy is a fact and that other fields of what is called parapsychology deal with real phenomena and not imaginary ones? These ideas so completely violate accepted canons that few psychologists are willing even to consider the possibility that they may be true. No mechanism has yet been found by which such phenomena may be analyzed or explained in familiar terms. Radically different views as to the nature of space and time may have to be developed to deal with phenomena like these, and the theory of relativity has made a beginning at this difficult task. Scientists brought up in the older modes of thinking will hardly blaze fresh trails here, and the establishment of new ideas may have to await the passing of their opponents from the scene.

* * * * *

The biological goals in protoplasm merge imperceptibly from bodily regulation through instinct and feeling into those aspirations that mark man at his highest. They make him a different sort of creature from a merely intellectual animal—a desiring, questing, creative being. He stands on the frontier between material and spiritual things. His spirit so transcends all else in him that it intrigues us with the hope that perhaps it even may partake of an infinite Spirit suffusing the universe itself. It reaches down into the depths of life and makes there, many are convinced, a *direct* contact with reality that seems to tell us more about our own nature and our relation to the universe than intellect alone can ever do. From it pour up into consciousness a throng of emotions and desires that are the most significant part of our experience and persuade us, mysterious though they are, that underneath the physical senses and the reasoned processes of mind there is a region full of richness, meaning, and truth but accessible only by this pathway

(*See inside back cover*)